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BIOLOGY AND EXPERIMENTATION[1]

Opening a new laboratory for experimenting on living things gives a thrill to any one who has pursued that adventurous occupation. The typical experiment on living things, according to the maxim of the older zoologists, is to kick a dog; the outcome is likely to be stirring, it may be astonishing and perturbing. And as biological experimenters we are in the blessed time of youth; we have not gone far enough to know what to expect. Great regions are still almost without a preliminary survey. General principles are still unsettled. Anything may happen.

What shall we try to do in our new laboratory? Where can we best take hold? What may we hope to accomplish? Why do we work by experimentation? What *is* experimentation, indeed? What are its foundations, its principles? What must we look out for in experimenting on living things?

In thinking over these questions, it helps to look over the experiences that zoologists have had so far as they have gotten in experimenting on things alive. Although intentionally or unintentionally men have always experimented with living things, the use of experimentation as a systematic method of research in zoology, employed on a large scale, is very recent. But even in that brief period, we *have* found out something about the peculiarities of experimentation on living things; about how to experiment and how *not* to experiment. Application of experiment to living things turns out, with a thorough-going consistency, to be itself a great experiment; a proceeding by trial and error, like that of a rat in a maze. To learn how to experiment, the only method *is* to experiment; to make errors, and then later to avoid the errors. The errors are an essential part of the process; no errors, no advance. But after they are made they must not be repeated; no elimination of errors, no advance. And to eliminate them we must mark them.

Living men, here present, can remember when zoologists did not work by experimentation. When I became conscious of the science, zoologists were doing descriptive work, and drawing far-reaching conclusions from that. Mainly these conclusions were as to the course that had been followed in their evolution by particular animals and by particular systems of

[1] Address at the dedication of the Whitman Laboratory of Experimental Zoology at the University of Chicago, June 4, 1926.

organs. But at a certain period, within a few years, almost every one stopped that, and turned into experimental work. The reason for that is a pretty reason; and worthy of meditation. About the course followed in evolution by particular kinds of animals, they had drawn conclusions that were impressive; these they had developed into histories as fascinating as the myths of the gods of Greece and Rome; we may find them still in the journals of that period. But on comparison, it was found that they were not all in a tale; in fact their tales disagreed radically. They tried for a long time to convince each other, but failed. And the reason was that there was no way of deciding which, if any, of the tales were correct. But what hath the man of science of all his labor and of the vexation of his heart, if it leads to no general agreement, to nothing that can be demonstrated? And so the zoologists gave it up; they looked upon the works that their hands had wrought, and behold all was vanity and vexation of spirit. Henceforth, they said, we must so work that our results and conclusions can be tested; can be verified or refuted. We must be able to say: Such and such things happen under such and such conditions, and if you don't believe it you may supply the conditions, you may try it for yourself, and you will find it to be true. But that is precisely experimentation; and so they flocked with enthusiasm into experimentation.

So *that* is what we are doing when we experiment; at least it is one thing we are doing; we are trying to get results and conclusions that can be verified by repeating the essential conditions.

How men fared in the first pursuit of this idea will tell us something further about biological experimentation. When they gave up trying to establish the course followed in evolution by particular organisms, they had the world before them. What should they do next? There was a feeling of relief, of expansion. Says an investigator of that time, von Uexküll, "When in biology one has freed himself from the notion of evolution—a notion at last hunted to death—so that one is again in a position to look upon each animal as a unity complete in itself, instead of as the last chance product of an ancestral series that has been speculated together—then form and function gain a new interest and a heightened brilliance." This and similar pronouncements, which were not uncommon for a time, were meant, I take it, not as denial of the fact of evolution, but as expressions of relief that one may now think and work on something else.

And so work scattered in many directions. Some attacked by the new methods the question of how the egg develops into the adult; others studied behavior; others the internal physiology of organisms; others heredity and variation. Enthusiasm was at a high pitch, that at last the right way to work had been found; now the way was open for steady advance.

But in that advance there were unexpected adventures; some of the sort to remind you of the traditional biological experiment of kicking the dog. The main body of workers attacked the problem of development from the egg. We can't, they said, establish the course by which the organism has developed in its evolution, for that is past and gone. But we certainly can find out just how and according to what laws it develops from the egg: how it differentiates into diverse parts, diverse tissues and organs, and produces an adult; for we have that process before us and can experiment on it. For this movement a dashing leader arose in young Driesch, a brilliant investigator and analyst; he became the voice of the zoological modernists, their high priest and prophet. Analytical experimentation, he proclaimed, is the one and only possible way of salvation for biological science; the one and only way of getting that knowledge of the *causes* of things which constitutes science. He not only preached, but practiced; the appearance of a new experimental paper by Driesch was the sensation of the times.

Now mark what happened to Driesch and to his fellow enthusiasts in experimentation! They were trying to find out how the egg develops into the adult with its diverse parts, and to so do this that their results should be verifiable. There were clearly two possibilities. One was that the egg contains a lot of diverse things, a lot of determinants, each of which produces one of the later organs or tissues or parts of the body, so that the egg is a mosaic of diverse parts. These are distributed during development to the different cells, to the different regions of the body. As a result, one region produces head, another body; one region produces the right half of the animal, another the left; one part produces an eye, another the heart. If development is thus by distribution of these diverse parts, then perhaps one could take the egg to pieces and get the pieces to develop. In that case each will produce its own part; we shall have separate right and left halves, separate heads, arms and legs scattered about in our experimental hatcheries.

The alternative possibility is that the egg is not composed of any such diverse parts, and that the way it develops and what it produces depend on the relations of the different regions to each other, and to the surroundings. The parts fit themselves to the situation and work harmoniously together, so as always to produce a unified organism. Separate halves, separate heads and legs would be an absurdity.

Experimentation was bound to tell which of these possibilities is correct. Separate the parts of the developing egg—say the first two cells; or destroy one,

and see what the separate cells will produce, a half animal or an entire one. Here is a plain and simple question so put to nature that she is bound to give a plain and simple answer, which can be verified by anyone who will repeat the experiment.

The difficult experiment was carried out. Roux did it with the amphibian egg and found that the first alternative was the correct one; a half egg produces a half animal; each region produces its foreordained part; the mosaic idea is verified. Driesch did it, with the sea urchin egg, and found that the second possibility was the correct one; a part of the egg produces the entire animal; any part can produce any part of the animal or the whole animal. Obviously one or the other was mistaken; to find out which, many investigators took a hand, trying it on many different organisms. Some found that Roux was right, some that Driesch was right. Years passed, with acrimonious controversy. Then Driesch, along with Morgan, finds an animal in which Roux's results are correct. Enormous sensation! Others find that in one and the same organism, sometimes Roux's result is correct, sometimes Driesch's.

And this was the general upshot. Some organisms operated in accord with one of the alternatives, some with the other; some parts of an organism in accord with one, some with the other. What one animal or one part couldn't do another could. All the conflicting reports were correct. The situation was that of the Gilbertian comic opera chorus, "For you are right and I am right and he is right and all is right."

But what can you make of a verdict like that? Where is your clear, definite and verifiable answer that experimentation was to give? What advantage does it show over the old methods of work? What's to be done about it?

Look what *was* done. Driesch, the apostle of experimentalism, gave it up, finally and completely; he turned fundamentalist. He decided not only that we do not know how the egg develops, but that now we know that we never can know, in experimental terms. Conditions discoverable by experimentation are not what determine the happenings in living things. The experimental method is not adequate to biological reality; it is fundamentally a failure. Driesch withdraws from it, and attempts to get at the nature of reality by other methods.

And so that's *one* thing that may happen to the biological experimenter! But must it happen? And if not, why not? *Is* experimentation essentially inadequate, a failure, as applied to living things?

Suspending for a time these questions, what did the rest of the experimenters do? They didn't despair so quickly as Driesch. By refined methods, by centrifuging and the like, they rearranged the parts of the developing egg. The results became still more incomprehensible and confusing. And then, along about 1910, with a few exceptions, they quit. From that time, for a long period, experimental contributions on the nature of development from the egg almost disappear from zoological publications. Most of the other experimenters didn't follow Driesch into fundamentalism, but they decided that, practically, experimentation on development from the egg didn't lead now to concordant and intelligible results that we could all agree on. They had gotten into a blind alley, and they backed out.

And so that is another thing that may happen to the biological experimenter!

What was the trouble in all this? What was really found out in this campaign? We discover that to plain and simple experimental questions we do not always get plain and simple experimental answers. We discover that general principles which beforehand seem obvious may be shown by the event to be wrong. We discover that when we demand of nature on which of two mutually exclusive alternatives she operates, she shows us some operations on one, some on the other, some on a mixture of the two. We find that we can not naively transfer the results and principles that we obtain by experimenting on one organism to another organism; or even to another part of the same organism. We find that what one organism can't do, another can. We find that what a given organism doesn't usually do, it may do when put to it.

But these are maxims of anarchy, of denial. They appear to vindicate Driesch, the fallen angel of experimentalism; they appear to justify his henchman, von Uexküll, who declares that there is an unresolvable contradictoriness in biological nature.

For any constructive suggestion from all this, the best that the invincible optimist can do is to moralize somewhat as follows: For rules or principles of general aplication, we can not naively generalize the responses given to our experiments by the first organism we work on, or by any single organism. The method of crucial experiments is a snare and a deception. If we are to get truths of general validity, we must compare the answers given by many different organisms, to many different experiments, and seek for some principle that includes as special cases all the discordant details.

But is there really hope in such a quest? What have the further adventures of experimentalists to say on this?

There came early into experimental zoology a powerful impulse from another body of workers. These had already developed in biology another experimental discipline, one at that time curiously limited in its objects of work, in the factors with

which it dealt, and in its outlook; but a really experimental science; namely, what was called physiology. This dealt mainly with the chemistry and physics of the substances found in organisms and with the action of recognized chemical and physical agents upon them. These two categories of things were urged upon experimental zoology as the essential and exclusive material for experimentation. This impulse centered largely in the study of movements and behavior. Other experimental zoologists had attacked the question: What makes organisms behave as they do? What makes them go in a certain direction? What makes them gather in a certain region? The physiologists set forth that organisms are masses of a certain kind of material, to-wit, protoplasm. Men had already experimented on the movements of masses of matter, and had learned generally applicable principles concerning them. Masses, both organic and inorganic, are impinged upon by physical and chemical agents from the surroundings: by light, heat, chemicals, electricity. The masses are moved by these agencies, in ways depending upon the nature of the particular agent. Argal, the way to understand the behavior of organisms is to find out just what external agents or forces are impinging on them, the directions from which these come; and the particular movements produced by each agent. Then the mathematical resultant of these movements is what we call the behavior of the organism.

This chain of reasoning contains true elements of permanent value. One of the fundamentals of biological experimentation *is* to know and control the environmental agents. But look now at the results of its application. The program was carried out. Crucial experiments were performed. Favorable masses of organic substances—lower organisms that could be had in large numbers—were subjected to light, to heat, to chemicals, and the like. The results, on the grouping of the masses, and on the direction of motion required to produce this grouping, were determined and classified. This yielded the tropisms: simple, direct, uniform movements. By application of this concept of tropisms the movements of organisms were explained; their goings out and comings in, their gatherings and groupings, their behavior. A triumph of the clear and simple expositions that come from really enlightened work in experimental science.

But mark again what happened. Certain investigators desired to know the "particular go" of these things. They studied, under the influence of single agents, single individuals of particular kinds of lower organisms, one after another, following the details of their motions. The movements were not simple and uniform. Different species under a given impinging agent acted quite diversely, as do dogs and cats and squirrels; each had his specific way of responding. Different methods of response were correlated with different peculiarities of structure. How the organisms behaved depended, not alone on the agent impinging on them, and on the kind of material of which they were made, but also upon the way that material was arranged; as is true of a bell or a typewriter or an automobile. These arrangements were varied and numerous; the result was not uniformity of behavior, but heterogeneity and variety. We have come here upon another principle that is of fundamental significance for the biological experimenter. The physics of diverse ***arrangements*** of substances is as essential as the physics of the homogeneous substances taken singly.

And a still further fundamental principle showed itself in this work. Even the single individuals, acted upon by a single agent, as a diffusing chemical, did not move uniformly and directly, like iron filings under pull from a magnet. On the contrary, each went through a number of diverse motions and moved in many directions, before they all had gotten collected in a certain region. The final result was due to the cessation, the elimination, of many of the motions; and continuance of others. The essential question becomes: What causes the elimination of the motions that cease? We have come upon another one of the things that play a major rôle in biological experimentation: the phenomenon of selective elimination.

Now in dealing with this subject of behavior, I realize that I am treading on a lava stream still hot from the fires of controversial eruptions, so I will hasten to step off. Arrived at a safe distance, I wish to try to point out certain general features in the landscape. The history just set forth appears typical for biological experimentation. At first the general features, the beginning and the end, of "crucial" experiments on "typical" organisms form the material of our science. The science is now clear, uniform, simple, intelligible. Then other organisms are examined, and the steps intervening between the beginning of the experiment and its final result are studied. As this is pursued, the critical and decisive parts played by diversity of arrangement, of constitution; and by selective elimination, become manifest. Variety, diversity, takes the place of uniformity and simplicity. Such has been the story in other fields; for example, in the progress of genetics, the study of heredity and variation. At first there are laws of inheritance, abstract and mathematical; they hang in the air. These laws as they are followed become more varied, more arbitrary, more unintelligible. And then it is found that their form and content is the resultant of the operation of special arrangements of the organic ma-

terial; certain systems of structure, the chromosomes. Where these arrangements are different, the rules of heredity are different. These rules become intelligible only through understanding these arrangements and their operation. And further, the processes of genetics reveal themselves as the production, on an immense scale, of diverse combinations, diverse systems, giving the widest scope for selective elimination and selective persistence. It is a history which, with variations in detail, has unrolled in many fields of experimental zoology.

And now that we have gotten in hand at least a few of the main threads that weave themselves into the complex tissue of biological experimentation, let us look at that tissue; let us examine the interweaving of the threads that make it up; let us weigh the significance of each.

Two of the fundamentals for biological experimentation were, as we saw, from the beginning emphasized by the physiologists who did so much to promote experimentation in zoology. The first is the analysis of the environment. We must know the outer agents that act upon organisms; we must study and control them in detail; we must be physicists and chemists for their sake; we must experiment analytically with them; we must know their effects on organisms, singly and in combination. Here is one of the most extensive fields for experimental zoology; here particularly is one of its great opportunities for influencing the practice of human life. Agriculture, hygiene, medicine, are largely outgrowths of such work. A laboratory of experimental zoology must be a laboratory for control of the environment.

Yet this is not the only requirement. Biological experiments that limit themselves to analyzing the environment and cataloguing the immediate effect on organisms of its components, will not lead far into biological science. Only when combined with adequate consideration of the other fundamentals does it become an instrument for such insight; without this it may be and has been an instrument of deception.

The second fundamental, likewise emphasized by the early physiological impulse, is the study of the physics and chemistry of the substances that make up organisms; the study of colloids; of nuclear compounds; of secretions; of hormones; of tissues. This is so fully recognized and so practically established that there is a special type of institution for it; laboratories of physiological chemistry. I need not dwell upon it here; it is now riding the crest of the advancing wave.

The third fundamental is logically but an aspect of the second, an aspect of the physics of the organism. But it requires separate consideration, both because of its extreme importance for zoological experiment; and because it was minimized, nay, despised and rejected of the physiological impulse in zoology. This is the rôle of physical arrangements of material in organisms; gross physical arrangements as well as minuter ones; what is variously called organization or structure. Structure had become the object of one of those epidemic phobias that beset scientific men as they do other men. In the days before experimentation, zoologists had given a romantic and mystical turn to the phenomena of structure in organisms; they built upon it a great edifice which was called morphology. They discovered in organic structure plans, styles, comparable to the diverse styles of architecture; to Gothic, Romanesque, Classical and the rest. But the physiologists said: This may be pretty, but is it Science? It is not. Out with it. We shall have nothing to do with morphology; it is fantastical. And throwing away the baby with the bath water, they largely rejected also the rôle of structural arrangements, even in experimentation. This it was that led to most of the adventures or misadventures of the sort I have recounted, in the progress of experimental zoology. It is important that this phobia should no longer dominate our work. Consider for a few moments the rôle of arrangements or organization in experimental work, and the consequences of its neglect.

Structural arrangement, organization, is of course physics; we find it playing a very great rôle in physics as that science advances. The properties of atoms depend upon the arrangement of the electrons; of molecules on the arrangement of atoms, of crystals on the arrangement of molecules. In organisms there is a great extension of this. They are bodies in which the arrangements have become complex and differentiated, and have passed into the grosser, the visible features as well as in the finer details. They are bodies in which there is an almost infinite variety of these arrangements, as we pass from species to species. They are systems of structures. In consequence, their properties and the way they respond to experiments, depend largely on these systems.

This puts a very great limitation upon the adequacy of experimentation and conclusions when only our first two fundamentals are taken into consideration. Knowledge and control of the environmental agents impinging upon organisms, and of the physics and chemistry of the separate substances of which they are composed, does not suffice for understanding what happens in them. For, the same materials, under the action of the same agents, respond in most diverse ways, depending on how the materials are arranged. This is as true for physics as it is for biology. The same lot of materials, under the action of the electric current, may in one arrangement act as a clock and tell time; in another act as a typewriter and spell

John; in a third act as a computing machine and give the product of 9 times 17. A laboratory of experimental zoology is a very museum of such diverse arrangements, responding diversely to the same conditions, with no necessary corresponding diversities in the constituent materials, aside from their arrangements. As a result, the responses of organisms need not at all correspond in their diversity to the diversities of the agents which act upon them.

Again, physical arrangements are readily made which, like an organism, may respond in one and the same way to the most diverse and opposed agents. Such a one may react in some single way—as by ringing a bell, or lowering a window—to heat and to cold, to acid, to alkali and to neutral salt; to mechanical shock, to light and to electricity. The world of organisms is a world of such arrangements. It will not do therefore, as has been so often done, to take the gross responses of such arrangements as typical and general for the substances concerned, irrespective of their arrangement; as yielding in that sense a general physiological or physical law. The axoltol transforms under the influence of the thyroid secretion. It has an adequate amount of that secretion in active condition. But it does not transform, for some special arrangement prevents the secretion from coming to action. Of such is the kingdom of organisms.

How far may we trace the decisive rôle of arrangement of parts? It is as pronounced in most lower organisms as in higher ones; the responses of infusoria are the workings of complex systems, diverse in different species. Is it true for Amoeba? For the fluid protoplasm within cells? May these move and react diversely, in different instances, as their structure is diverse? May there be as great diversities in the finer details of structure as in the grosser ones—so that different instances of protoplasmic flow may be as diverse as locomotion by legs and by wings? All this appears possible. Is there indeed a limit to this? Are there any properties of organisms in which special arrangements, organization, play no part? These are unsettled questions, but of the greatest importance for the experimental biologist.

It is largely the habitual neglect—nay, the contemptuous rejection—of these relations by some biological experimenters, that has so often led the experimental method to grotesque failure where triumph was expected. This is what has so often led the biological experimenter into a land of Cockayne; a land of romantic and unsubstantial phantoms, as mystical as any creations of the older morphologists; and fading away at the touch of reality. This it is that led to the tragedy of Driesch; it is the different structural arrangements in diverse organisms that bring about their diverse responses to the experiment of separating the parts of the egg.

In general, it is to this decisive rôle of diverse arrangements that are due the seemingly anarchistic principles which we deduced from the early experiences of experimenters. To it is due the fact that we can not directly transfer the experimental results that we have gotten in one field to another field. We can not transfer them uncritically even from one organism to another. And *a fortiori* it will not do to transfer uncritically the results of experimentation on inorganic things to organisms; the arrangements of parts are different. To this is due the maxim that what one organism can't do, another can; a maxim verified in every field of experimentation. To this is due the deceptiveness of the method of crucial experiment so much employed—the single experiment that is to give a generally valid answer to a question proposed. It is largely because of this that it is only through comparison of experiments on many diverse organisms that we can hope for truths of general validity.

But can we hope for truths of general validity? If we must stop with the truth that organisms are diverse arrangements, and therefore act diversely, respond diversely, is not biology incurably pluralistic, a heap of heterogeneous details? If we must stop there, surely it is. To picture it so, as Driesch urged in rejecting diverse arrangements as the explanation of the discordant results of experimentation, is merely to photograph the situation with all its complexities; is merely to state at once all the difficulties that we are working to overcome. A solution can lie only in accounting for the diversity of arrangements, of structures; in discovering how arrangements are changed, how new arrangements are produced, how from one arrangement come many. The problem of evolution we have thrown out of the window and we have locked the door, but it returns at the keyhole. To discover how organisms come to be arrangements; to be diverse arrangements; to discover how organic arrangements are produced and transformed and differentiated and conserved, is the final, the fundamental problem for the biological experimenter.

Changes of arrangements, of structure, we find as we experiment; these must be our port of attack. Some of these changes of arrangement seem stereotyped and automatic, like a shift in a typewriter, causing it to print capitals in place of lower case letters; a mere working of the mechanism already existing. So we may find in behavior that an infusorian responds to a continued stimulus by a whole series of motions, one touching off the next; inorganic arrangements acting in this way through the principle of the shift are readily made.

So, too, do we find a chain of diverse arrangements produced in the development from the egg. We start with a complex mechanism, the chromosomes with their many diverse substances or genes systematically

arranged; the cytoplasm; the environment. The system so operates as to change its own organization, and thereby its own responses to experiment. Development of the individual is a gradual series of transformations of the arrangement of parts; hence showing a series of diverse responses to given conditions, to experiment. Different organisms begin as different systems with different kinds and degrees of arrangements of their parts, hence they respond diversely to experimentation. Thence it was that arose the troubles and defeats of the early experimenters on development.

Again, in genetics, in the processes occurring at the production and union of germ cells we have a continual and kaleidoscopic production of new arrangements, new combinations, occurring in a systematic and perhaps predictable manner, as the working out of existing mechanism.

But in these cases where transformation seems but the working out of a complex mechanism already present we do not feel sure that we are getting light on the production of structure where it did not before occur: the change from one structure to another in a way not stereotyped. How does this occur? We find it in several sets of phenomena. During the lifetime of the individual we find it in what we call the formation of habits. Here is an actual change of organization that so far as we can see is not stereotyped; not a repetition of what has before occurred. How does it take place? We do not know. Here is fundamental work for the experimenter.

Changes of organization are induced too when we subject the developing organism to special or unusual conditions; a head may be caused to appear where a tail should have occurred, and similarly of other induced changes; they may extend to the fine details of organization. In such changes of organization we seem to approach the final secret of biology.

But the changes we have mentioned disappear with the individuals in which they have occurred. They do not, so far as we have been able to discover, produce alterations in their descendants; "acquired characters are not inherited." And therefore they do not account for the permanent differences of system that we find among organisms; it is these permanent, these hereditary, diversities that form the deepest problem of biology.

In some way such lasting diversities of organization do occur. All different animals, or a very great number of them, are originally pieces of the same material; the animal kingdom is essentially one organism which, like a mycetozoan, becomes separated into small pieces. As time has passed, these pieces have transformed; transformed in their most intimate nature, so that the substances which make up the germ cells—what we call the genes—have become diverse. For formerly these genes and germ cells all produced organisms much alike. Were they Amoebas, perhaps? But now some of them still produce Amoebas, some of them crayfish, some tape worms, some frogs, some black birds, some horses and some men. How and according to what rules have these changes occurred? How and according to what rules are they still occurring?

Experimenters that are at work upon this question are engaged upon the most fundamental of all the questions of biology. Other aspects of biological experimentation are of extreme interest, theoretical and practical. We must know the environment and the effects it produces on organisms; the diverse effects it produces on diverse organisms. We must know the chemistry and the physics of the diverse substances that make up organisms. We must know how these are arranged; how they are combined into systems. We must know the diversity of these systems in different organisms; and how this results in diversity of response to environment, to experiment. But finally, we must know how it happens that organisms are diverse systems of structures and functions; what are the laws of the production of these diversities. Only this knowledge can bring the whole of experimental biology to a unity.

Having examined as it were a map for biological experimentation, let me turn for a moment to another one of its fundamentals; something not to be located at a particular region of the map; but pervading the whole; something inconspicuous, impalpable, yet potent, in physics, in chemistry, and above all in biology; "a mighty darkness filling the seat of power." This is what I touched briefly in the experiences of experimentalism with behavior; it is the operation of selective elimination, with its complement, selective persistence. This is the very Mephistopheles of biological experimentation, filling it, if unrecognized, with chimeras and deceptions. In our experimental material, many diverse combinations are formed, diverse chemicals, diverse motions, diverse genes, diverse systems. Experimental conditions cause the elimination of some of these, while others persist. At the end the material before us has changed. Our experimental agent appears to have worked a transformation; in fact it has worked only an elimination. If we do not see the details, the production of many combinations, the elimination of certain sorts, we shall enunciate laws of action, of transformation, that are delusive phantasms. Again and again has this happened in biological experimentation. Stock is subjected to given environment. After a few generations it is found to be changed; the change is inherited even upon restoration to the usual environment. Behold!

We have discovered the inheritance of acquired characters. And then selective elimination is found lurking beneath the surface, and we know not what we *have* discovered. But for it, the inheritance of acquired characters has been overwhelmingly demonstrated. It is the very evil genius of the biological experimenter. To it are due the teleological fantasies of biology. To it are due specific false doctrines in many concrete fields of work. Wherever in experiments there is superabundant production, whether of motions, of chemicals, of genes, of germ cells, of individuals, so that only a part continue—beware, for in such does the demon of selective elimination lurk. Particularly in the fundamental problem of biological experimentation—the formation and transformation of biological systems—does it play the master rôle.

The experimenter who is not perpetually conscious of it and of the possibilities of its action is in danger. To ignore it, as many have done, is to court disaster. It must be dealt with explicitly; it must be seized and controlled; it must itself be made the subject of investigation; only thus is there security. That this is difficult does not make it the less necessary.

The opportunity before the new generation of biological experimenters, those that shall work in this laboratory, is an enticing one. The first generation of experimenters in zoology were ill prepared. Those of us who came from the older zoology were hampered by inadequate preparation in the first two fundamentals—in the physics and chemistry of the environmental conditions, and of the organic materials; this has been a heavy handicap. Those who entered experimental zoology from physiology were equally hampered by inadequate appreciation of the second two fundamentals—the great and decisive rôle of diversities of organization; and the equally great but insidious rôle of overproduction with selective elimination; the taboo placed by the physiologists upon these things has been to them a heavy handicap. The new generation need suffer under neither of these handicaps; it can deal adequately with the one pair of fundamentals without failing to deal adequately with the other.

To such, to men who will have done with taboos and phobias, who will be physicists and chemists without failing to be also zoologists, the field is ripe to the harvest. To lay out a specific program for an experimental laboratory is the function of those that shall work therein. But a glance at some large features of the concrete situation, at the opportunities before us, is not foreign to our purposes.

Study of the environmental components and their effects on the organism; and study of the physics and chemistry of the organism are bound to form a large part of the concrete work of any experimenter. Carried out with enlightenment and with thoroughness they lead into every problem of biology; they will cast light upon every problem. This work is in full swing; I need not dwell upon it.

Genetics has of late been one of the most fruitful fields for experimentation, starting from the question of the distribution of inherited characteristics. With Morgan, American experimenters can say with pride that *that* part of the problem is in principle solved; and by the work of Americans.

But how the genes operate to produce the results that they do; how they interact with each other, and with other components; how they interact with the environment; in a word, how development occurs, from the egg to the adult—this is the field that is now open for conquest. At its first attack on this, biological experimentation, as we saw, fell back repulsed; its approaches had been ill prepared and unsystematic. Now a secure foundation has been laid by the work in genetics. Intermediate products, between the genes and the later characteristics, are laid bare by the work on hormones. Through the work of Spemann, of Child, of Harrison; through that of Lillie and Moore, and of others, the walls of the fortress have been undermined or demolished; the way is open. An immense amount of experimental work is yet required, and it must lead to truths of fundamental significance. A more inviting prospect can not be imagined.

On the still more fundamental problem of the production of permanent alterations in stocks; of permanent alterations in genes, the way seems to me less clear; this matter has the allurement of difficulty as well as of importance. Lasting alterations of genes, of stocks, have been observed under experimental conditions, but their causation, their physiology, and their relation to the general transformations of stocks are obscure or totally unknown. How does it happen that in different organisms the same diversity of characteristics is produced in one case by environmental differences, in another by gene differences? This is true even for so deep-lying a feature as the diversity of sex; it seems to be true for every kind of characteristic. *Is* there in some way a transition from environmental determination to gene determination? And if there is, how is it brought about? What *is* the relation of environment to changes of genes, to changes of stocks? It is little short of a scandal that we know so little of this; so little even of the problem of the direct injury of genes by environmental conditions. To such a body of evidence as Kammerer presents for the inheritance of acquired characters we can respond with little more than a gesture of incredulity; or with vague suggestions as to selective elimination. What *is* the rôle of selective

elimination in all this? What the rôle of mating, of biparental reproduction? The problem of the relation of environment to changes in stocks is one on which depends the answer to many pressing human problems; at the same time it is the one that contains the key to the unity of biological science. This question alone might well constitute the program of a great experimental institution.

H. S. JENNINGS

THE JOHNS HOPKINS UNIVERSITY

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE MILLS COLLEGE MEETING OF THE PACIFIC DIVISION

MILLS COLLEGE proved to be an ideal place to hold the tenth annual meeting of the Pacific Division of the American Association. The accommodations were excellent in every respect. Large auditoriums for the general sessions, well lighted and ventilated class rooms for the meetings of affiliated societies well met the purely physical needs of the convention. The cordial hospitality and thoughtful attention of the faculty and officers of the college, combined with the delightful environment of trees and glades, quaint architecture, flowers and sequestered paths made the occasion one to be very pleasantly remembered. The spirit of aspiring young womanhood seemed indeed to pervade the place. While the sciences figure prominently in the instruction offered at Mills, the inscription over a monumental doorway in its beautiful art gallery perhaps sounds the keynote for the harmony which prevails throughout the campus, "Art remains the one way possible of speaking truth"—a sentiment which at first thought might give a scientist pause, but with reflection and in such surroundings would be sure to win his assent.

The total registration was 402. While the attendance was largely drawn from the membership in the Bay region, including Berkeley, Oakland, San Francisco and Stanford, analysis of the balance shows a geographic distribution as follows: Northern California, outside of Bay district, 47; Southern California, 65; Canada, 2; Hawaii, 2; Mexico, 2; Nevada, 9; Oregon, 16; Philippines, 1; Utah, 3; Washington, 4. Besides, attendance was registered from Delaware, Illinois, Iowa, Massachusetts, Michigan, Minnesota, New Jersey, New York, Vermont, Washington, D. C., China, Egypt, England, Germany, Ireland, Russia and Sweden.

RESEARCH CONFERENCE

The general sessions in which the entire convention participated opened with the research conference at luncheon on June 16. The relation of the college to research was discussed. President Aitken presided and introduced the following speakers:

Professor Howard E. McMinn, Department of Botany, Mills College.

Professor Albert Schneider, dean of the School of Pharmacy, North Pacific College, Oregon.

Professor Vernette L. Gibbons, Department of Chemistry, Mills College.

Professor Philip A. Munz, Department of Botany, Pomona College.

Stress was laid upon the importance of inciting interest in research work among undergraduates, and various methods by which this could be done were advanced by the speakers.

SYMPOSIUM ON THE CONSTITUTION OF MATTER

Following the luncheon, adjournment was taken to Lisser Hall, where the symposium on "The Constitution of Matter" was presented. The various phases of this fascinating subject were discussed and recent contributions to the solution of the problem were described and interpreted in a series of four papers as follows:

(1) "The Elements and their Composition." DR. T. R. HOGNESS, of the University of California, Chemistry Department, Berkeley, California.

(2) "Atomic and Molecular Structure." DR. HERTHA SPONER, of the Physical Institute of the University of Göttingen, Germany.

(3) "The Nature of the Atom as explaining and as exhibited by Lines in the Stellar and Solar Spectra." DR. H. H. PLASKETT, of the Dominion Astrophysical Observatory, Victoria, British Columbia, Canada.

(4) "The Structure of Matter as elucidated by X-Rays." MAURICE L. HUGGINS, Department of Chemistry, Stanford University, California.

ADDRESS OF THE PRESIDENT

The address of the retiring president, Robert G. Aitken, was given on the evening of June 16.

Following a graceful address of welcome by President Aurelia Henry Reinhardt, of Mills College, to which response was made on behalf of the membership by Vice-president Joel H. Hildebrand, President Robert G. Aitken, associate director of Lick Observatory, delivered a scholarly address on the "Solar System: Some Unsolved Problems."

Prefacing his remarks with a plea for better instruction in astronomy in the secondary schools, urging that "every child has a right to be introduced to the stars as ever present friends" the speaker advanced to his theme, in which he showed a fine appreciation of the requirements and limitations of a popular address on an abstruse subject. He spoke of the

emphasis which had been placed upon stellar research during recent years as a natural consequence of the development of spectroscopy and photography. The inference which might be drawn that there was little more to learn about the solar system had led him to call attention to the many unsolved problems pertaining to the sun and its satellites. Dr. Aitken's entertaining and instructive presentation of this subject will be read with interest by the entire membership when published in SCIENCE.

Following the address of the president, a public reception was held in Mills Hall.

The lecture of Dr. L. O. Howard, Thursday evening, June 17, on "Insects and Human Progress" was well attended and his films, illustrating the depredations of insects and the method of combatting them by aeroplanes, proved to be a feature of the meeting.

DR. ARTHUR A. NOYES ELECTED PRESIDENT OF THE PACIFIC DIVISION

At the annual meeting of the members held immediately following the lecture of the evening on June 17, announcement was made of the election by the executive committee of Dr. Arthur A. Noyes, director Gates Chemical Laboratory, California Institute, as president of the Pacific Division for the ensuing year.

The election of a member of the executive committee being then in order, it was proceeded with and resulted in the selection of Dr. Leonard B. Loeb, professor of physics, University of California.

The general sessions closed Friday evening, June 18, with an address by Dr. W. F. Durand, president, American Society of Mechanical Engineers, on "Science and Civilization." It is hoped that Dr. Durand's very significant address may find early publication in SCIENCE. The dependence of civilization on the progress of science and the necessity as the great aggregate of scientific knowledge accumulates for some super science, some science of the use of science to correlate the ever-increasing body of information, so that discoveries of nature's operations in one sphere of research may be fully interpreted in their applicability to the solution of problems in apparently unrelated spheres; this organization of "a science of the use of science" was Dr. Durand's main thesis in this very thoughtful paper.

Before the adjournment of the Mills College meeting the executive committee canvassed the question of where the next annual meeting should be held. Tentative agreement was reached that Reno, Nevada, would be an appropriate location, providing the necessary arrangements could be made. Subsequent correspondence has brought a cordial invitation from President Walter E. Clark, of the University of Nevada, which has been definitely accepted. Announcement of the date of the Reno meeting will follow in due course.

Fourteen affiliated scientific societies held meetings at Mills College under the auspices of the Pacific Division as follows:

American Association of Economic Entomologists, Pacific Slope Branch
American Chemical Society, California Section
The American Physical Society
American Phytopathological Society, Pacific Division
Astronomical Society of the Pacific
Botanical Society of America, Plant Physiological Section
Cooper Ornithological Club
The Ecological Society of America
Pacific Coast Entomological Society
San Francisco Aquarium Society
Society of Experimental Biology and Medicine, Pacific Coast Branch
Western Psychological Association
Western Society of Naturalists
Western Society of Soil Science

Reports of these meetings are presented herewith so far as they have been received from the secretaries.

AMERICAN ASSOCIATION OF ECONOMIC ENTOMOLOGISTS, PACIFIC SLOPE BRANCH

(Roy E. Campbell, *Secretary*)

The eleventh annual meeting of the Pacific Slope Branch, American Association of Economic Entomologists, was held on June 16 and 17, 1926, at Mills College, California. The meeting was very successful, not only in excellence of papers and wide range of subjects discussed, but also in attendance. A total of seventy-nine members and visitors registered, which exceeds any previous meeting.

Thursday's sessions were held at the University of California in Berkeley. In the afternoon there was a symposium on "The Fundamental Value of Life History Data," being a joint session with the Pacific Coast Entomological Society. The value of life history data was emphasized to the systematist, in biological control and in economic entomology. A motion picture on the Mexican bean beetle was shown by Dr. L. O. Howard, and one on malaria by Professor Herms.

Friday's sessions were held at Mills College in Oakland. The outstanding session of the entire meeting was the symposium on "Petroleum Oil Sprays in Insect Control," at which papers were read by H. J. Quayle, R. S. Woglum, E. R. de Ong and E. J. Newcomer. Some very interesting data were presented, not only on the effectiveness of the highly refined oils

as insecticides, but also the possibility of injury to foliage and fruit, especially the danger of lowering the quality and delaying ripening and coloring of the fruit.

The meeting concluded with a dinner Friday night, at which the principal speaker was our president, Arthur Gibson.

Officers elected for the ensuing year were: *Chairman*, R. W. Doane, Stanford University; *Vice-chairman*, R. S. Woglum, Los Angeles, California; *Secretary-Treasurer*, Roy E. Campbell, Alhambra, California.

AMERICAN CHEMICAL SOCIETY, CALIFORNIA SECTION

(George S. Parks, *Secretary pro tem*)

The California Section of the American Chemical Society held a session at 2:00 P. M., Thursday, June 17, 1926, in the Chemistry Lecture Room, Mills College. Professor W. H. Sloan presided. A total of about forty attended the meeting.

The following eight papers were given:

The Activity Coefficient of Soap Solutions. By MERLE RANDALL, J. W. MCBAIN and A. M. WHITE.

The Transport Numbers of Protein Solutions in Dilute Alkali. By DAVID M. GREENBERG.

Studies in Creatine and Creatinine Excretion. By VERNETTE GIBBONS.

The Equilibrium between Isopropyl Alcohol, Acetone and Hydrogen. By KENNETH M. KELLEY.

The High Temperature Equilibrium between Zirconium Oxide and Carbon. By C. H. PRESCOTT, JR.

The Constitution of Ramie Cellulose. By W. H. DORE.

Action of Bacteria on Mineral Oils. By JOHN W. BECKMAN.

The Crystal Structure of Resorcinol. By M. L. HUGGINS.

The papers proved to be very interesting and provoked a good deal of discussion. On the whole they indicated a marked trend toward the more general employment of physico-chemical methods, especially in the study of problems in organic and biological chemistry.

THE AMERICAN PHYSICAL SOCIETY

(P. A. Ross, *Acting Secretary*)

The 140th meeting of the American Physical Society was held at Mills College, Oakland, California, on June 17, 1926. At the morning session Dr. Evelyn Aylesworth, professor of physics, Mills College, presided. The attendance was about fifty. In the afternoon a joint session was held with the Astronomical Society at the Chabot Observatory. Professor P. A. Ross presided.

ASTRONOMICAL SOCIETY OF THE PACIFIC

(C. H. Adams, *Secretary*)

By the courtesy of Director Linsley, the three sessions of the Astronomical Society of the Pacific (one, a joint session with the American Physical Society) were held in the convenient lecture room of the Chabot Observatory, which stands on the hill just above the college grounds. Professor Linsley described the educational work of the observatory, which belongs to the Oakland City School Department, in the opening paper on the first morning, Thursday, June 17, 1926.

The other papers presented covered a wide range, but dealt chiefly with stellar problems, only five of the twenty-three relating to bodies in the solar system, four of these to the sun itself.

The attendance averaged forty, including representatives of all the Pacific Coast observatories from Victoria, B. C., to Pasadena, several eastern astronomers and a number of amateurs. While all the papers were good and led to more or less animated discussion, particular interest attached to Dr. Hubble's two papers presenting the results of his investigations of the Non-Galactic Nebulae on the basis of photographs taken with the 100-inch reflector at Mount Wilson; to Mr. Pease's paper dealing with the possibility of constructing reflecting telescopes of much greater aperture than any now existing; and to Dr. Trumpler's paper on "Spectral Types in Open Clusters."

BOTANICAL SOCIETY OF AMERICA, PLANT PHYSIOLOGICAL SECTION

(O. L. Sponsler, *Secretary*)

The Plant Physiological Section of the Botanical Society of America held three sessions at Mills College, Oakland, California, on June 17 and 18. The sessions were well attended, about forty people being present at each session. Over thirty titles were presented, only a few of which were not read for one reason or another. Those which were read indicated a preponderance of interest in the more fundamental types of problems and displayed a critical attitude towards the more generally accepted notions in plant physiology. For example: (1) L. B. Becking, of Stanford, pointed out in "The Physical State of Protoplasm" that Brownian movement showed a heterogeneous state as regards viscosity of protoplasm and therefore quantitative statements of viscosity were meaningless; (2) W. Newton, Carnegie Institution Laboratory at Carmel, California, showed that absorption of CO_2 by the green leaf can not be interpreted on the basis of the Siegfried carbamino reaction, but that the absorption is due to the formation

of bicarbonates; (3) H. L. Van de Sande Bakhuyzen, of the Food Research Institute, Stanford University, from his investigations has decided that Robertson's formula for "autocatalytic reactions" does not hold for the growth of annuals, that the "relative growth rate" of Briggs is a variable product of two independent variables, and that the dry-weight ratio in plants, especially annuals, is dependent upon the stage of the life cycle in which it is determined; (4) studies of sap were reported by J. P. Bennett, Y. Milad and F. G. Anderssen on "Methods of obtaining Tracheal Sap," by F. G. Anderssen on "Analyses of Tracheal Sap," and by D. R. Hoagland, P. L. Hibbard and R. R. Davis, all of the University of California, on "Adsorption of Ions by Nitella cells"; (5) other attempts at gaining an understanding of protoplasmic activity were reported by George J. Peirce, of Stanford, by observations of "One-celled Algae living in Saturated Brine," "Support for the Electrostatic Theory of Permeability," by Oran Raber, University of Arizona (not read), "Fatigue of Chloroplasts," by R. M. Holman, University of California (not read), "Hydrogen Ions and Osmotic Pressure of Cell Sap," by Floyd W. Gail, University of Idaho (not read).

A number of papers were presented bearing more or less upon immediate practical application by the agriculturist or horticulturist. Among these were (1) studies on "Sterility of Developing Seeds," Katherine G. Bitting, and "Germination of Lettuce Seed," H. A. Bosthwick and W. W. Robbins, University of California; (2) influence of pruning on "Viability of Grape Pollen," A. J. Winkler, on "Quality and Quantity of the Wood of Resistant Vines," by L. O. Bonnet, on the "Composition of Dormant Pear Branches," by A. H. Elswy, all of the University of California; (3) of a somewhat similar nature are "Determination of Starch in Woody Tissues" by S. H. Cameron, "Changes in Pears indicated by Electrical Resistance," by L. P. Latimer, "Temperature Effects on Composition of Dormant Pear Branches," by F. E. Gardner, "Presence of Phloridzin in the Pear Tree," by F. B. Lincoln, "Lime-induced Chlorosis," by Y. Milad, all of the University of California; (4) salt requirements of plants were discussed with relation to the growth phase and to varietal characteristics by W. F. Gericke, University of California; "Soil Moisture in Relation to Growth of Fruit Trees," by F. J. Veihmeyer and A. H. Hendrickson, University of California. (5) "Effect of Smoke, Dust and Fumes on Vegetation" was discussed by H. de Forest in the absence of H. Severence, of the University of Southern California. "Ring Density of Sugar Beets as a Character for Selection" was presented by Karstner, of Riverside, for Dean A. Pack, B. P. I., Salt Lake City. "Relation of Storage Temperature to Dormant Period in the Potato Tuber," by J. T. Rosa, University of California, and "Extension of Pollen Longevity and Its Importance" by R. M. Holman, University of California (not read), conclude this group of papers.

Several other papers less readily classified complete the investigations reported. These are "Behavior of Oxidase System in Fruits," by W. V. Cruess, University of California, and two which were not read, "Water Relations of Bog Plants," George B. Rigg, University of Washington, and "Studies of Xerophytic Ferns" by F. L. Pickett, State College of Washington.

The retiring chairman and secretary are W. W. Robbins and O. L. Sponsler. The latter was elected chairman for the ensuing year. J. P. Bennett, University of California, Berkeley, was elected secretary. The meetings next year are to be held at Reno, Nevada.

Cooper Ornithological Club, Northern Division

(Hilda W. Grinnell, *Secretary*)

The June meeting of the Cooper Ornithological Club, Northern Division, was held on Thursday afternoon, June 17, 1926, at two o'clock at Mills College, California. President Amelia S. Allen presided, and forty members and guests were present. The reading of all minutes was omitted. The first paper of the afternoon was read by Mr. Ralph Hoffmann upon "Courtship Performances of Birds." Mr. Hoffmann has recently spent much time in the field and presented to his hearers many original facts concerning the spring activities of the following thirteen birds: the western grebe, pigeon Guillemot, black tern, Forster tern, Beal petrel, ruddy duck, Wilson phalarope, Wilson snipe, sage grouse, marsh hawk, Texas nighthawk, three-toed woodpecker and the sage thrasher.

Dr. Tracy Storer's paper was upon "Range Extension by the Western Robin." A definition of the former range of the robin in California, supplemented by records taken during the last ten years, showed a very decided increase in summer range. A review of conditions necessary for the successful rearing of broods of young robins pointed, according to Dr. Storer, toward the increased area of well-watered lawns in city parks and private gardens as the main factor concerned in the increased summer population, robins nesting always by preference near damp meadows containing an abundance of soft food for the young.

At the close of the meeting, a brief business session was held.

PACIFIC COAST ENTOMOLOGICAL SOCIETY

(Roy E. Campbell, *Secretary pro tem*)

The first session of the meetings of the Pacific Coast Entomological Society was held on Wednesday morning, June 16, at Mills College, California. In the absence of both the president and secretary, Professor W. B. Herms and Mr. Roy E. Campbell were elected to temporarily fill these offices. Mr. O. H. Swezey gave an informal talk on insect problems and entomological work in the Hawaiian Islands, mentioning particularly the efforts to prevent the introduction of injurious insects, and to introduce beneficial parasites. Mr. C. J. Drake spoke of the entomological problems of Iowa, mentioning in addition to troubles from insects the difficulties encountered because of the large number of fake remedies offered to the public.

Dr. G. Steiner, of Washington, D. C., gave a very interesting talk on the nematode parasites of insects, mentioning especially those of grasshoppers and mosquitoes. Possibility of controlling injurious insects by these nematodes was indicated.

On Thursday afternoon there was a joint symposium at Berkeley with the Pacific Slope Branch, American Association of Economic Entomologists, on the subject "The Fundamental Values of Life History Data." Papers were presented by E. C. Van Dyke and H. S. Smith.

The sessions closed on Friday night with the entomological dinner, which was attended by thirty-five persons. Talks were given by Mr. Arthur Gibson, of Ottawa, Canada, Mr. C. J. Drake and Mr. O. H. Swezey.

SAN FRANCISCO AQUARIUM SOCIETY

(Ethel Seale, *Secretary*)

A meeting of the San Francisco Aquarium Society was held at Mills College, June 17, 1926. The meeting was well attended. An address was given by Perry Clark, president of the society, on "The Care and Planting of Balanced Aquariums," and was demonstrated by a number of the most important water plants. President Clark also told of his treatment of sick fish with various antiseptics. The talk was especially valuable to people who have aquariums in their homes. An address was also given by Alvin Seale, superintendent of the Steinhart Aquarium, on the subject "Outdoor or Garden Pools." The address was illustrated with very fine lantern slides taken in various parts of California. Mr. Seale spoke on methods of construction, planting and care of garden pools and told of their value from an educational standpoint and the pleasure to be derived in building one.

The program closed with a moving picture reel, entitled "In a Drop of Water," illustrating the lower forms of animal life to be found in a drop of stagnant water.

SOCIETY FOR EXPERIMENTAL BIOLOGY AND MEDICINE, PACIFIC COAST BRANCH

(T. D. Beckwith, *Secretary*)

It was reported by G. J. Peirce, of Stanford University, that the heliotactic response of organisms living in saturated brine varies with the species. Studies are under way dealing with the resistance of these organisms to the crushing forces of crystallization. It is hoped these will give results regarding the ability of this protoplasm to withstand such physical forces. There is a very definite relationship of the pituitary body to growth of the animal. In addition, the removal of the hypophysis produces serious atrophy to the thyroid, parathyroid, adrenals and testes. Growth in animals which have lost the pituitary by operation may be restimulated by daily injections of material derived from the gland from normal animals of the same kind. The basal metabolism of these animals also varies from that of normal animals. These facts were reported by P. E. Smith and G. L. Foster, of the University of California.

C. A. Kofoid, E. H. Wagener and E. A. Allen, of the University of California, stated that cultures of Entamoeba dysenteriae, which is the organism of amoebic dysentery, may be maintained readily upon a medium composed of egg albumin plus certain nutrient salts. One strain has been carried through 286 generations to date. Blood should be added to the medium, but this must be derived from an animal which may be experimentally infected with the organism, as for instance, the rabbit. Cultures thus derived contain many bacteria, but the protozoan forms thrive among these. Inasmuch as certain human individuals give a Schick negative reaction which indicates that they are immune to diphtheria, even though no antitoxin may be demonstrated in their blood serum, evidence is presented that immunity to diphtheria in part, at least, is not antitoxic. Cellular immunity thus seems to play a part according to W. H. Kellogg, of the California State Hygienic Laboratory and University of California.

The enamel of the tooth does have within it a small amount of organic material, as has been demonstrated by T. D. Beckwith and A. Williams, of the University of California. By means of a technique which has been elaborated sections of guinea pig enamel matrix may be cut. A number of points regarding the morphology of these structures based on this method of attack were brought out by means of lantern slides. The Ehrlich theory of immunity is no

longer tenable, according to the researches of W. H. Manwaring, of Stanford University. This appears to be demonstrated by means of various perfusion experiments carried out by him. C. H. Danforth, of Stanford University, says that the Y-gene of the mouse produces fat and yellow hair. By genetic methods this action may be controlled. The genes do not interact to produce a specific type of protoplasm.

That tyramine has a muscular component in its effect upon the rabbit, cat and dog was stated by M. L. Tainter, of Stanford University School of Medicine. Hitherto, its action has been considered to be sympathetic only. Experiments are being carried on by F. DeEds, L. W. Empey and W. H. Farr, Stanford Medical School, in an endeavor to determine the nature of the changes induced by anaphylactoid agents in the blood in vitro. Acacia or gelatin added to blood in the test tube lower the surface tension. Viscosity is increased and the albumin globulin ratio increases. The red blood corpuscles sediment with increased ease. C. H. Thienes, of Stanford University, reported that curare, a poison for which hitherto no remedy has been known, may be opposed in the body by use of Congo Red. The action of the dyestuff upon the poison is not chemical but is physiological in nature. C. H. Thienes and P. J. Hanzlik state that the results on excised intestine and uterus of different species and under different conditions generally indicate pure muscular actions of cocaine and procaine. The responses vary with the degree of tonus. Therefore, these drugs are not sympathominetic.

A. Schneider, of the North Pacific College at Portland, believes that respiration tests may be used as a substitute for the usual basal determinations and that the pulse rate reduction has a certain value in measuring the action of digitalis.

L. B. Becking, Janet Plowe, Bing Moy and John Sapero, School of Biology, Stanford University, have measured the spectrum characteristics of some of the purple bacteria. A new form, also, was described by them. Their researches show that the present classification of this class of organisms is as yet very incomplete.

Convicts at San Quentin Penitentiary have been used as the basis of a survey of mouth protozoa carried on by C. H. Hinshaw, of the University of California. As the depth of a pyorrhoeal pocket increases, the likelihood of infection by Entamoeba gingivalis is increased. This form and the Trichomonas of the mouth have been cultivated in many different instances. It is interesting that the Trichomonas is definitely antagonistic to the entamoeba. F. Eberson, of the University of California School of Medicine, has shown the presence of heat sensitive skin reaction substances in the blood serum of tuberculous patients and guinea pigs and is not found in the normal animal. These do not behave like tuberculin. Titrations may be made by skin reactions and apparently the test has definite clinical value.

WESTERN PSYCHOLOGICAL ASSOCIATION

(Warner Brown, *Secretary*)

About sixty persons were in attendance at each of the four sessions of the sixth annual meeting of the Western Psychological Association. One session was devoted exclusively to animal psychology and one to clinical and test work. The papers presented at the two remaining sessions were of general interest to psychologists. A luncheon talk by Dr. Grace Fernald on the use of psychology in education was received with lively interest. A second luncheon talk which was to have been delivered by Superintendent Nelles, of the Whittier State School, had to be omitted on account of the illness of Mr. Nelles. On Friday evening the association received a word of greeting from the parent organization, the American Psychological Association, through its president, Dr. H. A. Carr. On that evening the retiring president, Dr. W. R. Miles, of Stanford University, presented a series of highly interesting motion pictures of the behavior of rats in a maze, demonstrating the advantages of motion photography in preserving records of actual behavior. He showed also pictures of the behavior of monkeys being experimented upon by Mr. O. L. Tinklepaugh at the University of California.

At the annual meeting officers were elected for the ensuing year: *President*, Dr. Kate Gordon, University of California, Southern Branch; *vice-president*, Dr. Karl T. Waugh, University of Southern California; *secretary-treasurer*, Dr. Warner Brown, University of California.

It was decided to accept the invitation of the University of California, Southern Branch, to hold the next meeting there, and probably in the latter part of June, 1927.

WESTERN SOCIETY OF SOIL SCIENCE

(H. S. Reed, *Secretary*)

The fourth annual meeting was held on June 15 at the University of California and on June 16 in conjunction with that of the Pacific Division of the American Association for the Advancement of Science at Mills College. The officers for the meeting were M. B. Thomas, *president;* R. E. Neidig, *vice-president;* and W. V. Halverson, *secretary-treasurer.*

A brief description of the papers read at the meetings follows in so far as abstracts could be obtained.

W. P. Kelley discussed the problem of ion exchange in soils with respect to base-unsaturation. It is evident that a soil in which part of the bases have been replaced by hydrogen ions must have a deficiency of bases. This condition affects the composition of the soil solution, the physical and other properties of the soil. The author mentioned made a study of methods for determining the base-unsaturated condition of the soil. Hissink's method of using $Ba(OH)_2$ gave unreliable results, but the electro-metric titration method, using the neutral point as the endpoint, gave a reasonably accurate measure of the replaceable hydrogen ions. Satisfactory results were also obtained by determining the amounts of NH_4 absorbed from NH_4Cl before and after treating the soil with $Ba(OH)_2$ or $Ca(OH)_2$. The exchange capacity of various American clay soils is low and not in proportion to their clay content.

J. F. Breazeale and P. S. Burgess presented the results of studies on the problem of phosphorus supply of the plant. It has been found that applications of acid phosphate result in better plant growth, but applications of insoluble phosphates, such as bone meal or floats, do not increase plant growth. The solubility of the latter was increased by the presence of CO_2 in the solution, but in calcareous or "black alkali" soil this factor is inoperative. If the soil contained hydroxides of iron or aluminum, there was a slow formation of ferric or aluminum phosphates from which the plants had more success in obtaining their supply of phosphate.

The effects of saline irrigation water upon the exchange complex of the soil mass were discussed by S. M. Brown. A degree of salinity sufficient to injure fruit trees may not affect the base exchange complex of the soil. The extent of the reaction is influenced not only by the concentration but by the relative amounts of the respective ions present. The reaction has very definite time and temperature coefficients and appears to be strictly chemical.

A. R. C. Haas and H. S. Reed described a characteristic foliage injury observed where citrus trees were grown in sand cultures receiving nutrient solutions of pure salts and irrigated with pure distilled water. As time went on the leaves were shed and the young twigs died back to the trunk. The condition appears to be due to the lack of some element not hitherto regarded as essential for tree growth. The affected trees recovered when a mixture containing small amounts of manganese, boron, titanium, strontium, ammonium, lithium, bromide, iodine and aluminum was included in the nutrient solution. The injury did not occur when trees were grown in soil, or in sand cultures irrigated with tap water.

Nitrification experiments made by W. V. Halverson showed the favorable effects of lime, sulfur, superphosphate and potassium sulfate. The most nitrate was found where the largest application of lime was made, even though that application was excessive. Soils on which crops were growing failed to show more than a trace of nitrate until the crop was harvested. The disparity in the efficiency of the nitrifying flora of different soils was pronounced but the rate of nitrification was also dependent upon soil conditions.

The recovery power of a soil after depletion of the plant food elements was described by J. C. Martin. A favorable moisture content was maintained and the soils held at a temperature of 27° to 29° C. The method seems promising for studying the potentialities of soils to restore the equilibrium after they have been cropped. The concentration of the soil solution increased more rapidly after corn or turnips than after oats had been grown. Continued incubation for five months showed that the concentration and composition of the soil solution reached levels which were very close to that attained in that soil after eight years' fallow in a container in the field.

The rôle of iron-depositing bacteria in the formation of hard-pan was discussed by C. S. Mudge, who has found the organisms in all specimens examined. Upon incubating samples for a period of two months in a medium consisting essentially of ferric ammonium citrate a typical precipitation of iron hydroxide was found. Examination of the sample under the microscope disclosed minute cavities within the pan structure itself, in which there were filaments of a microorganism resembling crenothrix. Such findings suggest the possible formation of insoluble iron salts within the soil which later might cause the pan formation.

The effect of paper mulches on soil temperatures was shown by experiments reported by Dr. Alfred Smith. During hot weather the soil one half inch from the surface of a bare plot was 10 degrees warmer during the day and 5.6 degrees cooler at night than on a plot covered with perforated black paper. The differences were less as the distance from the surface increased. The average night temperatures at the twelve-inch depth were highest under the black non-perforated paper and lowest under the gray perforated paper. The warmest soil during the week was that covered with black non-perforated paper and the coolest was that covered with gray perforated paper. The highest soil temperature found was 143 degrees Fahrenheit at a depth of one half inch in the soil of the bare plot. The maximum temperatures at a three-inch depth occurred two hours after the maximum

air temperature of the day, and the minimum soil temperatures also lagged about an hour and forty minutes behind the minimum air temperatures. At a twelve-inch depth there was a lag of eight hours in the case of the maximum and of six hours in the case of minimum temperatures.

The importance of the soil profile was emphasized by several speakers. C. F. Shaw discussed its importance as a basis for soil classification. He called attention to the necessity for recognizing the individual layers or horizons in drawing samples for any kind of analytical work. Plant roots range through the soil to a considerable depth; therefore the characteristics of the horizons in the root zone must be known and recognized.

C. F. Shaw and Alfred Smith presented results from which they conclude that surface evaporation can not pull water from an indefinite distance in the soil. It seemed that a soil like those they employed would lose no water by evaporation if the water table lay at a depth of ten feet or more from the surface.

An interesting session on Tuesday evening was devoted to "Research Methods on Soil and Plant Interrelationships." The more or less informal discussion was led by J. S. Burd and D. R. Hoagland and elicited many important suggestions.

At the business meeting, the following officers were elected: *President,* J. S. Burd, University of California, Berkeley; *vice-president,* R. E. Neidig, University of Idaho, Moscow; *secretary-treasurer,* H. S. Reed, Citrus Experiment Station, Riverside, California.

Following are the officers of the Pacific Division, American Association for the Advancement of Science, for 1926–1927:

President: Arthur A. Noyes, director, Gates Chemical Laboratory, California Institute of Technology, Pasadena, California.

Vice-President: Joel H. Hildebrand, professor of chemistry, University of California, Berkeley, California.

Secretary-Treasurer: W. W. Sargeant, California Academy of Sciences, San Francisco, California.

Executive Committee of the Pacific Division

Joel H. Hildebrand, chairman, professor of chemistry, University of California, Berkeley, California.

Arthur A. Noyes, director, Gates Chemical Laboratory, California Institute of Technology, Pasadena, California.

Walter S. Adams, director, Mount Wilson Observatory, Pasadena, California (1928).

Bernard Benfield, consulting engineer, Kohl Building, San Francisco, California (1929).

Leonard B. Loeb, assistant professor of physics, University of California, Berkeley, California (1931).

Ernest G. Martin, professor of physiology, Stanford University, California (1929).

Emmet Rixford, professor of surgery, Stanford University, 1795 California Street, San Francisco, California (1928).

J. O. Snyder, professor of zoology, Stanford University, California (1930).

O. F. Stafford, professor of chemistry, University of Oregon, Eugene, Oregon (1930).

W. W. Sargeant,
Secretary

THE INTERNATIONAL CONGRESS OF PLANT SCIENCES

It is now possible to make a more definite announcement regarding the program of the congress to be held in Ithaca the third week in August. Since interest centers largely in the speakers, a list of these will suffice to indicate the general character of the program.

At the opening meeting, to be held on the evening of Monday, August 16, it is expected that the entire congress will be welcomed by Livingston Farrand, president of Cornell University, and addressed by W. M. Jardine, United States Secretary of Agriculture. On Wednesday evening Professor F. A. F. C. Went, of the University of Utrecht, will deliver a public address on the subject "Plant Movement," and on Friday evening Dr. Erwin F. Smith, of the Bureau of Plant Industry, will speak similarly on "Fifty Years of Phytopathology."

Following is a fairly complete list of those who will participate formally in the programs of the various sections. Those whose names are starred are not expected to present their papers in person.

A. *Agronomy.* Zavitz (Guelph), Tulaikov (Saratov), McCall (U. S. D. A.), Marcello (Venice), Prianishnikov* (Moscow), Arrhenius* (Stockholm), Stoklasa (Prague), Love (Cornell), Christie* (Norway), Stadler (Missouri), Kiesselbach (Nebraska), Hayes (Minnesota), Leighty (U. S. D. A.), Holbert (U. S. D. A.), Jones (Conn. Agr.), Vavilov* (Leningrad).

B. *Bacteriology.* Winogradsky (Paris), Bergstrand (Stockholm), Mellon (Rochester), Henrici (Minnesota), Löhnis* (Leipzig), Buchanan (Ames), Waksman (Rutgers), Clark (U. S. Hyg. Lab.).

C. *Cytology.* Tischler (Kiel), Nemec (Prague), Seifriz (Pennsylvania), Guilliermond* (Paris), Taylor (Pennsylvania), Levine (New York), Goodspeed (California), Blackburn (Newcastle-upon-Tyne), Heilborn (Stockholm), Svedelius (Upsala), Harper (Columbia), Sakamura* (Sapporo), M. Nawaschin (Moscow), Ishikawa (Tokyo), Cleland (Goucher), Allen (Wisconsin), Sax (Maine), Randolph (Cornell).

D. *Morphology and Paleobotany.* Chodat (Geneva), Went (Utrecht), S. Nawaschin (Moscow), Porsch* (Vienna), Hoyt (Washington and Lee), Florin (Stockholm), Noé (Chicago), Jeffrey (Harvard), Eames (Cornell), Wieland (Yale), Mez (Königsberg), Svedelius (Upsala), Chamberlain (Chicago), Thomson (Toronto), Coulter (Yonkers), Buchholz (Texas).

E. *Ecology.* Rübel (Zürich), Skottsberg (Gothenburg), Cowles (Chicago), Conzatti* (Oaxaca), Domin (Prague), Szymkiewicz* (Lwow), del Villar* (Madrid), Pearsall (Leeds), Tansley (Cambridge), Toumey (Yale), Du Rietz (Upsala), Cooper (Minnesota), Nichols (Yale), Borza (Cluj), Palmgren* (Helsingfors), Szafer (Krakau), Maximow (Leningrad).

F. *Forestry.* Serpieri* and Pavari* (Florence), Jonson (Stockholm), Howe (Toronto), Toumey (Yale), Cajander* (Helsingfors), Endres* (Munich), Zon (U. S. F. S.), Fischer (Manila), Tkatchenko* (Leningrad), Petrini* (Stockholm), Rodger* (Dehra Dun), Dana (U. S. F. S.), Korstian (U. S. F. S.).

G. *Genetics.* Lehmann (Tübingen), Ernst* (Zurich), Tschermak* (Vienna), Sirks (Wageningen), Krenke* (Moscow), Blakeslee (Cold Spring Harbor), Malinowski (Warsaw), Shull (Princeton), East (Harvard), Muller (Texas), Eyster (Maine), Anderson (Michigan), Babcock (California), Davis (Michigan), Savastano (Acireale).

H. *Horticulture.* Chittenden (London), Macoun (Ottawa), Hansen (South Dakota), Jones (California), Kotowski (Skierniewice), Remy* (Bonn), Waugh (Amherst), Potter (New Hampshire), Hedrick (Geneva, N. Y.), Daniel* (Rennes), Overholser (California), Auchter (Maryland), Thompson (Cornell), Garner (U. S. D. A.), Hooker (Missouri), Carrick (Cornell), Anthony (Penn. State), Alderman (Minnesota).

I. *Physiology.* Lepeschkin (Prague), Ursprung* (Fribourg), Reed (Riverside), Lubimenko (Leningrad), Livingston (Johns Hopkins), Knudson (Cornell), Kostytschew* (Leningrad), Robbins (Missouri), Stoklasa (Prague), Scarth (McGill), Vouk (Zagreb), Crocker (Yonkers), Lloyd (McGill), Popoff* (Sofia), Swingle (U. S. D. A.), Nixon (U. S. D. A.).

J. *Pathology.* Quanjer (Wageningen), Murphy* (Dublin), Morstatt* (Berlin), Jaczewski (Leningrad), Whetzel (Cornell), Hollrung* (Halle), Shear (U. S. D. A.), Reddick (Cornell), Duggar (Mo. Bot. Garden), Klebahn (Hamburg), Earle (Washington, D. C.), Küster* (Giessen), Atanasoff* (Sophia), Walker (Wisconsin), Stakman (Minnesota), Allen (California), M. Löhnis (Schevingen), Jones (Wisconsin), Beauverie* (Lyons), Zweigbaumowna* (Skierniewice), Humphrey (U. S. D. A.), Nemec (Prague), Güssow (Ottawa), Van Slogteren (Lisse), Appel (Berlin), Butler (Kew), Foëx* (Paris), Barrus (Cornell).

K. *Pharmacognosy and Pharmaceutical Botany.* Rusby (Columbia), Zörnig* (Basel), Schneider (Portland), Klan* and Farwell (Parke, Davis & Co.), Wasicky* (Vienna), Plitt (Maryland), Viehoever (Philadelphia), Starker (Corvallis), Schwarz (Eli Lilly & Co.), Beal (Illinois), Gathercoal (Illinois), Zufall (Purdue), Hart (Columbia), Youngken (Boston), Tschirch* (Berne), Ballard (Columbia), Hogstad (Minnesota), Newcomb (Minnesota), Cortesi* (Rome).

L. *Taxonomy.* Ostenfeld (Copenhagen), Hutchinson* (Kew), de Wildeman* (Brussels), Sprague (Kew), Bailey (Ithaca), Hitchcock (U. S. D. A.), Briquet (Geneva), Hall (California), Illichevsky* (Poltava), Wiegand (Cornell), Hill (Kew), Fernald (Harvard), Abrams (Stanford), Rendle (British Museum), Nelson (Wyoming), Rydberg (N. Y. Bot. Garden), Skottsberg (Gothenburg), Schönland* (Rhodes Univ. College), Druce* (Oxford), Brand* (Sorau), Bonati* (Lure).

M. *Mycology.* Butler (Kew), Burt (Mo. Bot. Garden), Wollenweber (Berlin), Kauffman (Michigan), Guilliermond* (Paris), Shear (U. S. D. A.), Buller (Winnipeg), Brierley* (Rothamsted), Jaczewski (Leningrad), Wakefield (Kew), Ramsbottom (British Museum), Overholts (Penn. State), Cunningham* (Wellington), Arthur (Purdue), Klebahn (Hamburg), Faull (Toronto), Rea (Worcester), Jackson (Purdue), Dodge (U. S. D. A.), Fromme (Blacksburg), Mains (Purdue), Stakman (Minnesota), Wize* (Posen), Sartory* (Strasbourg), Dietel* (Zwickau), Siemaszko* (Skierniewice), Falck* (Hanover), Petrak* (Mähr-Weiskirchen), Kursanov* (Moscow).

Preliminary announcements of the congress have been mailed to all persons whose names appear in membership lists of American research societies in the plant sciences. All who contemplate attending the congress should return the reservation slips at once, in order that arrangements at Ithaca may be carried forward promptly and on an adequate scale. Any who have not received such slips should also engage accommodations in advance.

Response to the appeal for financial aid has so far been inadequate. Since the work of the organizing committee is almost wholly dependent on such aid, American plant workers are again urged to help. Any check, no matter how small, is a real contribution.

LESTER W. SHARP,
Secretary of the Program Committee
WILLARD STRAIGHT HALL,
ITHACA, N. Y.

SCIENTIFIC EVENTS

HELIUM IN THE NATURAL GASES OF JAPAN

THE thirteenth report of the Aeronautical Research Institute, Tokyo Imperial University, as summarized in *Nature,* contains an account of an examination of natural gases from Taiwan, Hokkaido, Honsyu and Kyusu, primarily for helium, by Messrs. Y. Kano and B. Yamaguti. The gas samples were taken from various sources, such as oil reservoirs, coal-mines,

mineral springs and volcanoes, and were analyzed for helium by Cady and McFarland's method, for other constituents by Hempel's standard method. The purity of the helium was tested spectroscopically. Carbon dioxide, sulphuretted hydrogen, oxygen, carbon monoxide, methane, ethane, nitrogen and heavy hydrocarbons were among the chief constituents of the gases investigated, and from the analytical results it has been possible to classify the natural gases into three types rich in carbon dioxide, in hydrocarbons and in nitrogen respectively; as in the case of helium in American natural gas, the percentage of this element is highest in nitrogen-bearing gas. The helium content of some mineral spring gases examined reaches 0.2–0.3 per cent., but unfortunately the amount of gas available from this source is strictly limited and insufficient for industrial purposes; very small quantities of helium were found in the gas from the Taiwan and Hokkaido areas, where it is associated with petroleum, the average being 0.005 per cent., again an impracticable amount for commercial purposes. The oil and gas reservoirs of Taiwan and Hokkaido are of Tertiary age, from which the low helium content of the gas from these sources is accordingly explained. The authors find that the percentage of helium in a hot spring gas depends to a certain extent on emanation content, though no direct ratio could be established, this agreeing with McLennan's conclusions with regard to Canadian natural gas. Samples containing no helium generally possess the least radioactivity, and this it is said to some extent supports the theory that the origin of helium in natural gas is to be ascribed to disintegration of radioactive substances; if this is so, then the geological age of a gas reservoir is an important criterion of helium possibilities.

RESEARCH FELLOWS IN MINING AND METALLURGY OF THE CARNEGIE INSTITUTE OF TECHNOLOGY

THIRTEEN graduates of colleges and universities have been appointed research fellows to conduct an extensive program of studies in mining and metallurgy at the Carnegie Institute of Technology during the coming year. The work, as in the past, will be carried on by the department of mining and metallurgical engineering, in cooperation with the Pittsburgh Station of the U. S. Bureau of Mines and under the direction of two advisory boards of engineers and business men representing the mining and metallurgical industries.

Of the thirteen investigators, five will study problems in metallurgy and eight have been assigned to carry on research in problems relating to coal mine engineering. The program of metallurgical studies will be supervised officially by an advisory board of Pittsburgh district steel men and engineers. The coal mining investigations will be made under similar conditions with an advisory board of coal operators and mining engineers.

Four of the studies will be financed by the Carnegie Institute of Technology, while the remainder of the work will be underwritten by the industries. Among the contributors are the American Gas Association, which is interested directly in a study of "warning agents for manufactured gas," The National Coal Association, the New York Edison Company and other industrial enterprises.

Senior investigators to assist the research fellows will be furnished by the Bureau of Mines. Problems have been selected and assigned for the year as follows: "Constitution of Low Temperature Tar," by B. F. Branting, fellow, and R. L. Brown, senior; "Coal Ash Fusibility as Related to Clinkering," by C. L. Corban and E. J. Talbert, fellows, and P. Nichols and A. Selvig, seniors; "Formation and Identification of Inclusion in Steel," by G. R. Fitterer, fellow, and C. H. Herty, senior; "Solubility of Iron Oxide in Iron and its Effect on Physical Properties of Pure Iron," by R. L. Geruso, fellow, and C. H. Herty, senior; "Flammation of Fine Sizes of Coal Dust," by C. H. Gilmour, fellow, and C. M. Bouton, senior; "Electric Power of Storage Batteries *vs.* Trolley Locomotives," by Donald C. Jones, fellow, and L. C. Illsley and C. W. Owings, seniors; "Warning Agents for Manufactured Gas," by E. R. Perry and E. J. Talbert, fellows, and S. H. Katz, senior; "Viscosity of Open Hearth Slag," by M. B. Royer, fellow, and C. H. Herty, senior; "Mine Timber Treatment," by N. A. Tolch, fellow, and L. D. Tracy, senior; "Distribution of Ferrous Oxide between Slag and Metal," by S. P. Watkins, fellow, and C. H. Herty, senior; "Case Carburizing of Steel," by R. E. Wiley, fellow, and C. E. Sims, senior.

MENTAL HYGIENE AT YALE UNIVERSITY

YALE UNIVERSITY has announced plans for the development of mental hygiene work in its academic community. With the aid of an appropriation of $50,000 a year for five years from the Commonwealth Fund the university will establish next fall instruction in mental hygiene and will provide a trained staff with whom students may advise concerning problems in this field.

In the freshman year voluntary conference groups will be arranged in which the point of view of mental hygiene will be presented and through which the students may become acquainted with the staff. The groups will be limited to fifteen or twenty men and will meet a member of the staff at least three times during the year.

It is expected to institute a lecture course open to

sophomores in Yale College and the Sheffield Scientific School which will aim to give the student a broad historic concept of life with emphasis upon the adaptation of the individual to his environment. An advanced course for seniors and juniors is also planned which will cover special problems associated with vocational aptitudes. In addition, general lectures to large numbers on various phases of mental hygiene will be given from time to time.

This program will be initiated next fall with an advisory committee in general control, a staff in residence and the assistance of visiting experts. The advisory committee is made up of Dr. Arthur H. Ruggles, who has been professor of mental hygiene at Yale this year; Dr. Edward A. Strecker and Dr. Frankwood E. Williams, consultants in psychiatry; Dr. Stewart A. Paton, chairman of the resident group in psychiatry and mental hygiene. Of these the new appointees are Dr. Williams, editor of *Mental Hygiene,* and medical director of the National Committee for Mental Hygiene; Dr. Paton, lecturer in neurology at Princeton since 1910; and Dr. Strecker, professor of nervous and mental diseases, Jefferson Medical College, Philadelphia. Dr. Williams and Dr. Strecker will not be in residence, but will be called upon for lectures and consultations.

The resident staff will be composed of Dr. Lloyd Thompson, for the past year clinical instructor in psychiatry and clinical director of the Connecticut Society for Mental Hygiene; Dr. Clement C. Fry, assistant in psychiatry at the Harvard Medical School, and Major Harry N. Kerns, M.D., formerly psychiatrist at the United States Military Academy and this year chief medical officer at the Brooks Aviation Field, Texas.

THE ROCKEFELLER FOUNDATION AND THE LEAGUE OF NATIONS

ACCORDING to the annual report of the International Health Board of the Rockefeller Foundation during the year 1925 the board continued to cooperate with the health section of the League of Nations through contributions toward the international interchange of public health personnel, and the epidemiological and public health intelligence service.

The program for international interchanges was started in 1922, and up to November, 1925, 388 health officials from forty-eight different countries had participated. These interchanges consist of courses of travel studies lasting about seven weeks and include lectures by the technical experts and responsible health officials of the countries visited, and also inspection trips. They are held in widely separated parts of the world and are attended by representatives from many countries.

Within the year, four such general collective interchanges were held in Great Britain, Belgium, Yugoslavia and Japan, and a fifth was arranged for a special group from Latin-America. This last-mentioned group made a tour which included visits to Cuba, the United States, Canada, and certain European countries. In addition to these five courses of travel study there were two specialist interchanges, one for factory sanitary inspectors and the other for port medical officers on the Mediterranean Sea. Individual missions were provided for health officials from thirteen countries.

The board continued its contributions toward the Epidemiological and Public Health Intelligence Service. The service collected and published current epidemiological information from all European countries except Albania and Portugal, from all North America and Australasia, and from practically all countries in Asia and Africa which issue such data. Special epidemiological and statistical investigations by individuals were completed and reports were prepared on the organization and work of the health services of various European countries. Handbooks on statistical services, monographs on health organization and special studies were published. Groups of experts continued to study methods of improving the comparability of international medical statistics and of standardizing statistical classification.

In April an interchange of vital statisticians was held under the auspices of the Epidemiological Intelligence Service. It was the third of these meetings for the study of medical statistics. Representatives from thirteen countries participated. The main subject under consideration was the classification of causes of death. Statistical offices in Denmark, Sweden, Norway, Scotland, England, the Netherlands and Switzerland were visited.

The outstanding event of 1925 in the development of the Epidemiological Intelligence Bureau was the establishment of an office in Singapore, to be known as the Far Eastern Intelligence Bureau. A preliminary conference, held in February in Singapore, was attended by representatives of twelve eastern governments. The bureau came officially into existence on March 1. Reports are telegraphed from the bureau each week. They contain information with regard to health conditions in forty-seven ports of the east. A monthly report also is published.

SCIENTIFIC NOTES AND NEWS

THE autumn meeting of the National Academy of Sciences will be held in Philadelphia, upon the joint invitation of the University of Pennsylvania and the Wistar Institute of Anatomy.

Dr. Simon Flexner, director of the Rockefeller Institute for Medical Research, New York, was elected an honorary member of the Société de Biologie of Paris at a meeting held on June 19.

Dr. Benjamin L. Robinson, Asa Gray professor of systematic botany and curator of the Gray Herbarium at Harvard, has been elected a corresponding member of the Botanical Society of Geneva.

Dr. Calvin W. Rice, secretary of the American Institute of Mechanical Engineers, who is visiting Germany, has been made an honorary doctor of engineering by the Technical Institute at Darmstadt.

The gold medal of the American Geographical Society was recently presented to Dr. Erich von Drygalski, professor of geography in the University of Munich and leader of the German South Polar Expedition of 1900 to 1903. The presentation was made by Dr. Jacob Gould Schurman, ambassador to Germany.

Corresponding members of the Vienna Academy of Sciences have been elected as follows: Dr. G. H. Hardy, professor of mathematics at the University of Oxford; Professor C. V. L. Charlier, the Swedish astronomer; Dr. Ramón y Cajal, professor of anatomy in the University of Madrid, and Dr. O. Richter, professor of botany in the University of Brunn.

Sir Ernest Rutherford (Cambridge), Sir Frederick Hopkins (Cambridge), Professor H. A. Lorentz (Haarlem) and Dr. H. L. le Chatelier (Paris) have been elected foreign members of the Polish Academy of Sciences, Cracow.

Dr. Max Freiderichsen, professor of geography in the University of Breslau and director of the Geographical Institute, has been elected a corresponding member of the Geographical Society of Finland.

We learn from *Nature* that Mr. G. S. W. Marlow has been released by the Association of British Chemical Manufacturers to devote part of his time to the appointment of secretary and editor of the Faraday Society and secretary of the Institute of Physics, pending the completion of final arrangements for carrying on the official work of these bodies. Mr. Marlow was assistant secretary to the Institute of Chemistry from 1919 until 1925.

Dr. Beaumont C. Cornell, of the Johns Hopkins Hospital, has been appointed a fellow at Duke University to conduct a research on pernicious anemia.

The new officers of the American Geophysical Union as elected for the period July 1, 1926, to June 30, 1929, at the annual meeting of the union in April last are: *Chairman,* H. S. Washington; *Vice-chairman,* G. W. Littlehales. (J. A. Fleming continues as general secretary through June 30, 1928.) The newly elected officers of sections for the corresponding period are: (*a*) Geodesy—*Chairman,* William Bowie; *Vice-chairman,* F. E. Wright (W. D. Lambert continues as secretary through June 30, 1928); (*b*) Seismology—*Chairman,* L. H. Adams; *Vice-chairman,* N. H. Heck (D. L. Hazard continues as secretary through June 30, 1928); (*c*) Meteorology—*Chairman,* H. H. Kimball; *Vice-chairman,* G. W. Littlehales; *Secretary,* A. J. Henry; (*d*) Terrestrial Magnetism and Electricity—*Chairman,* N. H. Heck; *Vice-chairman,* J. H. Dellinger; *Secretary,* J. A. Fleming; (*e*) Oceanography—*Chairman,* T. Wayland Vaughan; *Vice-chairman,* G. T. Rude; *Secretary,* Austin H. Clark; (*f*) Volcanology—*Chairman,* T. A. Jaggar, Jr.; *Vice-chairman,* F. E. Wright (R. B. Sosman continues as secretary through June 30, 1928).

To report scientific meetings in Europe this summer and to visit European correspondents and affiliated organizations in England and France, Watson Davis, managing editor of Science Service, sailed for Europe on July 17.

Dr. Douglas Houghton Campbell, of Stanford University, sailed on July 21 for England to attend the Oxford meeting of the British Association for the Advancement of Science.

Dr. Lewis R. Jones, professor of plant pathology at the University of Wisconsin, is spending the summer in Hawaii, upon the invitation of President Dean, of the University of Hawaii. The purpose of his visit is to join in conference with Professor F. C. Newcombe, formerly head of the department of botany at the University of Michigan, and others concerning certain diseases of the pineapple.

Dr. Alfred L. Kroeber, of the University of California, has been authorized by special decree of the government to work with scientific men of Peru in archeological explorations of the Inca regions. Archeological specimens will be distributed by the government between Americans and Peruvians in spite of a previous decree forbidding exportation of archeological specimens.

Dr. Robert Ballenegger, head of the college of horticulture of the University of Budapest, and one of the secretaries of the International Association of Soil Science, has been in the United States for several months as visiting professor in soils at Michigan State College. He was brought to this country under the auspices of the American-Hungarian Foundation. As an exchange professor Dr. H. J. Stafseth, of the department of bacteriology of Michigan State College, has been spending the past college year at the Royal Veterinary College in Budapest. Dr. Stafseth

was expected to return to this country the latter part of July, while Dr. Ballenegger returns to Hungary in August. During the latter part of May and the early part of June Dr. Ballenegger visited and gave addresses on the soils investigations in Hungary at Purdue University, the University of Illinois, the University of Missouri and the University of Nebraska. He also visited the Missouri Botanical Garden. In Nebraska he made a study of the work on soils being carried on by the university and the Federal Soil Survey.

PROFESSOR A. F. LEBEDEFF, of the University of Rostov, Russia, has arrived in this country for the purpose of carrying out certain investigations on nitrate reduction by bacteria and on the water movement in the soil. He is spending the first two months at the New Jersey Agricultural Experiment Station, at New Brunswick.

DR. EUGEN OBERHUMMER, professor in the University of Vienna and president of the German Geographical Society, will visit the United States in the autumn.

DR. JOHANNES WALTHER, professor of geology in the University of Halle, will be visiting professor at the Johns Hopkins University during the next academic year.

DR. GEO. W. MUHLEMAN, head of the department of chemistry at Hamline University, has been appointed a delegate from the American Chemical Society to attend the sixth Congress of Industrial Chemistry at Brussels which opens on September 27. He will spend the remainder of the year at the University of Geneva, working in the department of physiological chemistry.

DR. AND MRS. HARRIS H. WILDER, who have leave of absence from Smith College, sailed on July 17 for Genoa. Their address for the coming year will be care of the American Express Company, Naples, Italy.

DR. WILLIAM C. BOECK, who has been a research fellow in the department of comparative pathology of the Harvard Medical School, is visiting clinics and laboratories in Europe and after his return will begin an appointment in medicine at the Mayo Clinic.

SIR FLINDERS PETRIE has announced that he is abandoning his lifelong work on Egyptian excavations and is transferring his activities to Palestine because of obstacles placed in his way by French officials in the Egyptian antiquities department.

KNUD RASMUSSEN has sailed from Copenhagen to join the Palmer-Putnam Expedition at Etah, Greenland, which is collecting bird and animal specimens in the Arctic regions.

PREPARATIONS for an expedition under Dr. Lange Koch, to explore the east coast of Greenland, are being made by the Danish government. Dr. Koch proposes to spend a year between 70 degrees and 76 degrees north latitude mapping the geology of the region.

DR. JULIO SAVASTANO, son of Dr. Luigi Savastano, the plant pathologist of Italy and now in charge of the Italian citrus and fruit culture station in Sicily, is now working in the Bureau of Plant Industry studying under a scholarship granted by the International Education Board.

LECTURES on evolution to be given during the summer session of Columbia University include a course on "The Evolution of Man," by J. H. McGregor. "The Evolution of the Stars" will be the subject of a lecture by Professor Harlow Shapley, director of the Harvard Observatory. Addresses will be given by Professor Richard S. Lull, of Yale University, on "The Evolution of the Earth"; by Professor Henry E. Crampton on "The Evolution of Plants and Animals," and by Professor Edward L. Thorndike on "The Evolution of Intelligence."

CHARLES A. COFFIN, founder and former president of the General Electric Company, died on July 14 at his home in Locust Valley, L. I., aged eighty-one years. Mr. Coffin was president of the company for many years and was responsible for the organization of the research laboratory at Schenectady. In June, 1913, he became chairman of the board of directors and retained that office until 1922. When he retired a $400,000 fund was established to be known as the Charles A. Coffin Foundation, the income to be used to encourage the study and application of the science of electricity.

FRANK ADDISON WARD, for years president of the Ward Natural Science Establishment and son of former Mayor Levi Ward, has died at his Rochester, N. Y., home. He was born at Rochester in 1851 and, after graduating from Princeton, joined a cousin, Henry A. Ward, in the science establishment, then the only clearing house in the world for the collection of material necessary for the study of natural science. Later Mr. Ward became president of the Merchants Bank of Rochester, retiring to become chairman of the board.

OBERLIN SMITH, inventor of presses and dies, for sixty-three years president and principal owner of the Ferracute Machine Company, died on July 18, aged eighty-seven years. Mr. Smith was a fellow of the American Association for the Advancement of Science and a past president of the American Institute of Mechanical Engineers.

WASHINGTON A. ROEBLING, the engineer, builder of the Brooklyn Bridge, died in Trenton, N. J., on July 21, aged eighty-nine years.

A. M. HERRING, designer of airplanes, at one time associated with the late Professor Samuel P. Langley, died on July 17, aged sixty years.

PROFESSOR CLARENCE AUSTIN MORROW, of the division of agricultural biochemistry of the University of Minnesota, died on July 1, aged forty-five years.

FRANK MORLEY WOODRUFF, an authority on taxidermy, for many years curator of the Academy of Sciences and Museum of Natural History in Lincoln Park, Chicago, died on July 21, aged fifty years.

THE REVEREND T. R. R. STEBBING, F.R.S., the eminent zoologist and one of the foremost authorities on the Crustacea, has died at the age of ninety-one years.

THE death is announced of Professor Edward Babak, dean of the medical faculty and professor of physiology at Masaryk University, Czechoslovakia, on May 29, aged fifty-three years, and of Dr. F. B. Hofmann, professor of physiology at the University of Berlin, at the age of fifty-six years.

THE eleventh annual meeting of the Optical Society of America will be held at Philadelphia, Pa., Thursday, Friday and Saturday, October 21, 22 and 23.

THE Society of Chemical Industry held its forty-fifth annual meeting and Congress of Chemists in London, from July 19 to 23, in cooperation with the Chemical Society, Institute of Chemistry, Association of British Chemical Manufacturers, British Association of Chemists, British Chemical Plant Manufacturers' Association, Bio-Chemical Society, Coke Oven Managers' Association, Institute of Brewing, Institute of Metals, Institution of the Rubber Industry, Institution of Chemical Engineers, Institution of Petroleum Technologists, Oil and Color Chemists' Association, Society of Leather Trades' Chemists, Society of Public Analysts and the Chemical Industry Club.

THE Brooklyn Botanic Garden has succeeded in meeting the requirements of a pledge of Mr. John D. Rockefeller, Jr., to contribute $250,000 to the permanent funds of the Botanic Garden to be used for its educational and scientific work, provided the Botanic Garden authorities secure a like amount before the close of the year 1926. A small amount not subscribed has been underwritten by a friend of the Botanic Garden in order that the institution may benefit from the income of the entire fund for the remainder of the year. Contributions are still being received and it is hoped that the entire unsubscribed balance will be met in this way before December 31.

THE name of the chemical element whose symbol is Al is to be found in the literature spelled "aluminum" and "aluminium." The former spelling is almost universal in common usage whereas the latter spelling has predominated in scientific literature. The committee on nomenclature, spelling and pronunciation of the American Chemical Society, of which E. J. Crane, of the Ohio State University, is chairman, has announced that it favors the spelling "aluminum." This is in the interest of uniformity. The decisions of this committee are always considered as tentative for one year and criticism is welcomed.

IN the article on William Chauvenet, SCIENCE, page 24 (Vol. LXIV, No. 1645, July 9, 1926) replace the seventh line by the following words: "Chauvenet was born on May 24, 1820. As an only"

UNIVERSITY AND EDUCATIONAL NOTES

AT the commencement of Brown University it was announced by President Faunce that a million of dollars had been pledged to the endowment fund of the university, $500,000 given by the Aldrich brothers of Providence provided the university raised the same amount. The condition had been met.

AT the recent celebration of the bicentenary of the foundation of the medical faculty of the University of Edinburgh, the new rooms for the surgical department, fully equipped for research by a gift of $250,000 from the Rockefeller Foundation, were dedicated.

MEMBERS of the administrative board of the School of Public Health of Harvard University for the year 1926–27 are: President Abbott Lawrence Lowell, David Linn Edsall, Milton Joseph Rosenau, Ernest Edward Tyzzer, Edwin Bidwell Wilson, Hans Zinsser and William Lorenzo Moss.

PROFESSOR KURT KOFFKA, of the University of Giessen, has been appointed professor of psychology in the University of Wisconsin for the academic year 1926–1927.

DR. KARL HERZFELD, professor of physical chemistry in the University of Munich, has been called to a professorship in the Johns Hopkins University.

DR. HOMER B. LATIMER, of the University of Nebraska, has been appointed professor of anatomy in the medical school of the University of Kansas, at Lawrence, Kansas.

DR. E. A. BOYDEN, of the Harvard Medical School, has been appointed an associate professor of anatomy in the medical department of the University of Illinois.

THE department of biology of University College, New York University, announces the following additions to its teaching staff for the next academic year:

Dr. Richard P. Hall, Ph.D., University of California, and for the past three years at Rice Institute, Houston, Texas, to be assistant professor of micro-biology; Dr. F. L. Campbell, Harvard University, to be assistant professor of general physiology, and Mr. H. A. Charipper and Mr. F. J. Novotny to be graduate assistants in biology.

DR. RICHARD DOUGLAS PASSEY, lecturer in pathology in the Welsh National School of Medicine, Cardiff, has been elected to the new chair of experimental pathology in the University of Leeds and will also be director of cancer research. Dr. Bryan A. McSwiney, who was assistant professor of physiology at Trinity College, Dublin, and subsequently lecturer in experimental physiology at the University of Manchester, has been appointed to the chair of physiology, in succession to the late Professor W. F. Shanks.

DISCUSSION

GEOLOGIC AGE CALCULATIONS

IN view of the recent publication of two articles under the auspices of the Committee on the Extension of Geological Age by Atomic Disintegration in which the age of the Black Hills pitchblende is in one case referred to as fifteen hundred million years and in the other as sixteen hundred and sixty-seven hundred million years, it may be well to remark that the work of Davis[1] of course precedes that of Richards and Hall[2] and his estimate is the closer calculation, while the figure in the article by Richards and Hall is simply a round number to show the importance which this particular sample and the age determination thereof seem to have.

It may be well to say that even though no great exactitude can yet be claimed for the numerical figures for age, the relative ages are of great geological value and significance. In this case, for instance, a much greater age is indicated for the Precambrian age of the Black Hills than was previously supposed on what were realized to be very insufficient grounds.

The physicists are hardly yet agreed on the second figure of the numerical constants necessary to compute time from the lead: uranium ratio, and moreover it is by no means universally accepted that the rate of decay of radioactive atoms is uniform in time. Of the eight or more conceivable factors that might affect it, not all have been eliminated as unimportant within the ranges of conditions that occur in the outer rind of the earth.

It does not seem likely that any of them will affect the relative ages. If so it may probably be found out by comparing ages thus obtained with ages derived in other ways.

This has recently been done by A. Holmes.[3] In time we may hope to correlate the periodic and progressive, the hourglass and pendulum methods of estimating time.

In this work C. W. Davis has added important facts, for his paper includes not only an analysis of the Black Hills pitchblende, but that from Katanga, and of a mineral from Utah which may be a recent representative of the Swedish Kolm and the pre-Cambrian anthraxolites.

ALFRED C. LANE

[1] *Am. J. Science*, 1926, xi: 201.

[2] *Journal Am. Chem. Soc.*, 1926, xlviii: 704.

[3] *Geological Magazine*, Nov.–Dec., 1925, xii: 505–515, 529–544.

AERIAL MUSIC IN YELLOWSTONE PARK

THE very interesting article in your issue of June 11 on a mysterious sound heard in Yellowstone Park brings to mind the fact that I heard sounds like that described many times in July, 1890, when, in the company of Professor Edwin Linton, then of Washington and Jefferson College, Pennsylvania, I was making a biological study of the waters of the park under the auspices of the U. S. Fish Commission. Linton and I heard these sounds with wonder and delight in different places and under different conditions—once I remember when we were riding on horseback through the woods, but never when they could be explained as due to anything in our immediate neighborhood.

I was so interested in this perplexing phenomenon that I made the following footnote reference to it in my report of our operations, made to the commissioner, Colonel Marshall McDonald, and published in the Bulletin of the U. S. Fish Commission for 1891. Referring to Shoshone Lake, I wrote:

> Here we first heard, while out on the lake in the bright still morning, the mysterious aerial sound for which this region is noted. It put me in mind of the vibrating clang of a harp lightly and rapidly touched high up above the tree tops, or the sound of many telegraph wires swinging regularly and rapidly in the wind, or, more rarely, of faintly-heard voices answering each other overhead. It begins softly in the remote distance, draws rapidly near with louder and louder throbs of sound, and dies away in the opposite distance; or it may seem to wander irregularly about, the whole passage lasting from a few seconds to half a minute or more. We heard it repeatedly and very distinctly here and at Yellowstone Lake, most frequently at the latter place. It is usually noticed on still, bright mornings not long after sunrise, and it is always louder at this time of day; but I heard it clearly, though faintly, once at noon when a stiff breeze was blowing. No scientific explanation of this really bewitching

phenomenon has ever been published, although it has been several times referred to by travelers, who have ventured various crude guesses at its cause, varying from that commonest catch-all of the ignorant, "electricity," to the whistling of the wings of ducks and the noise of the "steamboat geyser." It seems to me to belong to the class of aerial echoes, but even on that supposition I can not account for the origin of the sound.

I am surprised that it has not been studied and understood before now and I hope that some of the many scientific men who doubtless visit the park every year may find in it an attractive problem for vacation study.

Stephen A. Forbes
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VARIATIONS IN COLORS OF FLOWERS

I have noticed for a good many years considerable variation in the color in the flowers of many of our native plants. The plum is now in full bloom. Within a few hundred feet of here there is a beautiful specimen along the roadside that the axe has spared so far. It has a decided shell-pink color, and is one of the finest native plum trees I have ever seen. A little further on, on the edge of the woods, is another plum tree with cream-colored flowers, and not very far from this tree one of pink color, almost the color of the peach blossom. I noticed recently near Green Bay, Wisconsin, also a plum tree of decided pink, but not as pronounced as the one mentioned above. Then we have those that are pure white and others with a light grey touch.

Our western crab apple, Pyrus Ioensis, usually shell pink, is very often found in full pink. There are some trees here of beautiful rose color, the finest rose I have ever seen amongst flowers.

In a ravine or blow-out in the Dune Country near Grand Branch, Michigan, the majority of flowering dogwood, Cornus Florida, are pink or rose, and I have been told that a few miles from there, there is another colony of the pink-flowering dogwood.

On a recent trip to Eastern Kentucky and Tennessee I did not notice a single pink-flowering dogwood except those in the gardens purchased from nurseries. Are these varieties in colors of the flowers due to natural hybridization or difference in soil? The plums I have mentioned all grew in clay, the pink dogwoods in sand, the dogwoods in Kentucky and Tennessee in clay or stony land. Whatever the cause, this natural variation gives the nurseryman a chance for a greater color variation for the gardens, and it seems to me this will in time partly settle the grievance amongst many of our people against the embargo. Perhaps the embargo is a good thing from more than one standpoint. It will give us a chance to look into native land and perhaps discover new beauty for our gardens, making them typical American.

Jens Jensen
Ravinia, Illinois

ZYGOPHYLLUM FABAGO IN THE UNITED STATES

In Science for June 25, 1926, the Syrian bean caper, Zygophyllum Fabago, which had been found at Mesilla Park, New Mexico, January, 1925, was given as a plant new to the United States. Mr. J. C. Buchheister, of New York City, found a few plants of this Zygophyllum on Port Morris ballast grounds, June 10, 1900. He sent specimens to Dr. J. N. Rose, who said it had not been reported before from the United States. I collected it also at Port Morris on Long Island Sound, Bronx Borough, New York City, June 5th and July 8th, 1901. All these collections are in the herbarium at Cornell University. Mr. Norman Taylor in his Flora of the Vicinity of New York, says that, "Tribulus terrestris L. and Zygophyllum Fabago L. have both been collected near the metropolis. They are very doubtfully established." The ballast grounds at Port Morris must have been destroyed and built over many years ago.

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Cornell University

SCIENTIFIC BOOKS

Bacterial Infection with Special Reference to Dental Practice. By J. L. Appleton. Published by Lea and Febiger, Philadelphia and New York, 1925.

The dental profession and others interested in the microbiology of the mouth have waited long for some one to do what Professor Appleton has done. But "Bacterial Infection with Special Reference to Dental Practice" will prove valuable, not only to those who are especially interested in oral infections, but also to physicians, bacteriologists and immunologists. It is notable among recent new text-books in bacteriology for its original discussions and for its excellent up-to-date analysis of the literature.

The book is divided into three parts. Part I deals with bacteria, their morphology, physiology and classification. There is a separate chapter on the relation of bacterial growth to oxygen supply and another on chemical disinfectants, in both of which emphasis is given to the peculiar problems connected with oral hygiene. One outstanding feature is the compilation of data on limiting and optimal hydrogen-ion concentrations of various bacteria; another is the author's treatment of sterilization by heat. Both Migula's classification (modified from Chester) and Bergey's classification are summarized, but while the author is

nominally committed to a preference for the latter, in spite of its bizarre features, he may perhaps be excused for failing to follow it throughout the text, for, if he did, much of the presentation would be unintelligible to the average reader without considerable effort.

Part II treats of infection as a concept, methods of disease transmission, the characteristics of infection, predisposing factors, types of infection, modes of disease production, the defenses of the host (humoral and cellular), resistance and immunity, and artificial immunization. The importance of disease carriers, both to the dentist and to his patients, is adequately emphasized. But the dental aspects of the problem in this portion of the book are really incidental; infection and immunity are presented from a broad scientific standpoint and the references to the dental applications do not obtrude but rather serve to round out the discussion of a subject in which this phase is usually neglected.

The special infections of the oral cavity are covered in Part III, with careful and fairly complete reviews of the bacteriology of the streptococcus-pneumococcus group, the oral spirochaetes, dental caries, periapical infections, pyorrhea alveolaris, Vincent's Angina, tuberculosis, syphilis, focal infections and the rare gonorrheal stomatitis. The final chapter is devoted to clinical dental bacteriology in an attempt to organize a satisfactory technic for this poorly developed field.

There are few adverse criticisms, and none serious. An obvious misprint, as on page 188 (lines 9–10), and the apparent misplacing of a few paragraphs, as at the bottom of page 194 and on page 195, can be corrected easily in a new edition.

The general makeup is pleasing. There are ninety-one engravings and five colored plates. It is the best general contribution to the subject of dental infections that has appeared since 1890, when W. D. Miller published "The Micro-organisms of the Human Mouth."

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SPECIAL ARTICLES

A PRELIMINARY NOTE ON THE ETIOLOGY OF VERRUGA PERUVIANA

In certain narrow valleys among the Andes Mountains, particularly between Lima and Oroya in Peru, a peculiar disease exists which has long been regarded with special interest not only by local physicians but by all workers in the field of tropical medicine. The lesions on the skin, as the name "verruga" suggests, have the form of nodules or warts. As a rule the disease is of chronic type, and there are evidences of systemic as well as local infection, *e.g.*, fever, which is usually mild and of intermittent or remittent character, and anemia of moderate degree. The eruption on the skin is often preceded by a period of acute high fever, during which anemia is very severe, and peculiar bacilliform microorganisms are demonstrable in the red blood cells. This condition, which is called Oroya fever, may also occur simultaneously with the eruption on the skin, or it may follow the local manifestations. For many years Oroya fever was regarded as the severe form of the disease and verruga a milder manifestation of the same infection, but this theory has been open to question since 1913, when a commission from the Harvard School of Tropical Medicine studied both conditions and pointed out that their frequent association in the same individuals and their similar curious geographical distribution did not necessarily indicate that they were caused by the same parasite. The Harvard commission concluded that two distinct diseases were involved, because (1) either condition might occur independently of the other, (2) the peculiar intracorpuscular parasites, discovered by Barton in 1906 and named *Bartonella bacilliformis* by the commission, were not found by them in cases of simple verruga, and (3) monkeys and rabbits were readily infected with verruga by direct injection of suspensions of nodular tissue from verruga patients, but inoculations of a monkey and rabbits with blood of Oroya fever patients yielded no results, notwithstanding the presence of the parasite in the blood injected.

In order to determine whether or not Oroya fever and verruga are actually different phases of the same disease, it was necessary to isolate the microbic incitant in each instance and make comparative studies of the morphology, cultural properties, pathogenicity, and immunological relations of the microorganisms. It was recently found,[1] as reported in this Journal, that the parasite of Oroya fever, *Bartonella bacilliformis,* could be isolated in pure culture, and that various manifestations of the disease could be induced in young *rhesus* monkeys by inoculation of the culture. It also developed in the course of the experiments that inoculation of the cultures of *Bartonella bacilliformis* into the cutaneous tissues of the monkeys induced at the site of injection a typical "verruga" indistinguishable from that observed in human beings or in monkeys directly inoculated with verruga tissues. *Bartonella bacilliformis* was demonstrated both in the skin lesions and in the erythrocytes of the experimental animals.

[1] Noguchi, H., and Battistini, T. S., Science, 1926, lxiii, 212; *J. Exper. Med.*, 1926, xliii, 851.

The results just described made it seem probable that *Bartonella bacilliformis* was the cause of both Oroya fever and verruga, and it was desirable that parallel cultural and experimental investigations be made with human verruga tissues. Such studies were rendered possible through the kind intermediation of Dr. E. Campodónico, of Lima. Subcutaneous nodules were excised under aseptic conditions from verruga patients in Lima, and portions were forwarded to New York for bacteriological and experimental study by one of us (N.), the clinical and pathological work being carried out by the other (H.) in Lima. What has been accomplished thus far may be summed up as follows:

(1) Two young monkeys (*Macacus rhesus*), inoculated with a saline suspension of nodular tissue from Case P. 5, manifested fever (104° F.) within 1 to 3 weeks after inoculation, and from the blood in each instance was isolated in pure culture a microorganism indistinguishable from the strain of *Bartonella bacilliformis* previously cultivated from the blood of an Oroya fever patient. (2) In one of the monkeys a hard nodule about the size of a walnut developed within seven weeks near the middle portion of the tail. The lesion consisted of highly vascular granulomatous tissue, rich in endothelial cells, and containing a considerable amount of fibrous connective tissue. Pure cultures of the same organism which had been isolated from the blood were obtained from the nodular tissue. (3) *Rhesus* monkeys inoculated with the cultures developed verruga lesions at the sites of intradermal injection and showed marked anemia. Characteristic bacilliform microorganisms were found in the red blood corpuscles, and pure cultures of what appears to be *Bartonella bacilliformis* were obtained from both skin lesions and blood. (4) Serological tests showed the verruga strain and the strain from Oroya fever to be closely related.

Clinical and pathological reports will be presented later, together with full details of the experimental work just outlined.

Hideyo Noguchi,
Oswaldo Hercelles

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ON THE EXTENSION OF THE DEBYE-HÜCKEL THEORY OF STRONG ELECTROLYTES TO CONCENTRATED SOLUTIONS

The Debye-Hückel theory[1] explains the observed deviations of a solution of a strong electrolyte from the ideal solution by the electrostatic forces between the dissolved ions, which give rise to an additional term in the expression for the free energy of the solution. This free energy term depends, among other things, upon the dielectric constant of the solution. In the original theory, which applies only to dilute solutions (up to about 0.3M), the dielectric constant is assumed to be independent of the concentration of the solution. Hückel[2] has given an extension of the theory to concentrated solutions, assuming that the dielectric constant decreases linearly as the concentration increases. Through a faulty application of the so-called charging process,[3] Hückel's expression for the electrostatic free energy term is erroneous, and his further results, which are all derived from this expression, are therefore invalid.

The writers have shown that the correct expression for the electrostatic free energy is the same as in the original Debye-Hückel theory, the only difference being that the dielectric constant is no longer assumed to be independent of the concentration. The writers have furthermore developed methods for calculating, from experimental data (freezing point and vapor pressure lowering, electromotive force, solubility) not only the free energy term referred to, but also the ionic radius and the change in the dielectric constant with changing concentration.

It is found that the ionic radius varies rapidly with the temperature, the radii for LiCl, NaCl, KCl at 0° calculated from freezing points being roughly twice those at 20° determined from vapor pressures.

The dielectric constant also varies markedly with the temperature. Moreover, at any fixed temperature, but with increasing concentration, the dielectric constant begins by decreasing much more (from 50 to 100 times) rapidly than Hückel's expression indicates. It soon reaches a minimum and starts to increase more slowly, until in very concentrated solutions it is considerably larger than in pure water. This behavior of the dielectric constant is in entire qualitative agreement with the direct measurements of Walden and his collaborators[4] of the dielectric constant of non-aqueous solutions of electrolytes.

It is also found that certain salts, for instance, KNO_3, do not conform to the theory, the probable explanation being that in addition to its electric charge, the NO_3 ion also carries an electric dipole moment. Theoretical investigations in this direction are now in progress.

T. H. Gronwall,
Victor K. LaMer

Columbia University

[1] Debye and Hückel, *Physik. Z.*, *24*, 185 (1923).

[2] Hückel, *Physik. Z.*, *26*, 93 (1925).

[3] Debye, *Physik. Z.*, *25*, 97, (1924).

[4] Walden, Ulich and Werner, *Z. Phys. Chem.*, *116*, 261, (1925).